

Psychological Review

EDITED BY
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SYMPOSIUM ON OPERATIONISM

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THE PSYCHOLOGICAL REVIEW

SYMPOSIUM ON OPERATIONISM

INTRODUCTION

About a year ago Professor E. G. Boring wrote to suggest a symposium on operationism and appended a few questions which he said he would like to have answered. The Editor welcomed the idea and invited a number of persons interested in operationism to take part in the symposium. They were sent Professor Boring's questions and asked to add questions of their own to the list. Professors E. G. Boring, P. W. Bridgman, Herbert Feigl, Harold Israel, Carroll C. Pratt and B. F. Skinner accepted the invitation and submitted additional questions. The complete list was then sent to each participant with instructions to write on any or all of the questions. When the galley proof was ready, all of it was sent to each person with the request that he write a short rebuttal. All but one participant consented. These replies, which are more than rebuttals, follow the main articles under the title 'Rejoinders and Second Thoughts.'

These are the questions. They are referred to in the text by their numbers.

1. (a) What is the purpose of operational definitions? When are they called for?

Since it is obviously impossible to explicate an operational definition for every construct-term used in scientific discussion, there must be some principle which determines when operational definitions are useful.

(b) Logically, operational definitions

could form an infinite regress, since the construct-terms used in describing an operation are themselves in need of definition.

How is this regress limited in scientific practice?

2. When the same construct is defined by two independent operations, should it be said that there are really two constructs? For instance, it has been said that tape-measured distance and triangulated distance are really two kinds of distance and should perhaps have different names.

Against this view it can be argued that these are operations for showing the equivalence of operations, *e.g.*, for demonstrating the identity of taped and surveyed short distances.

3. (a) Are hypothetical operations which are physically impossible with present available techniques, of scientific use? Is the other side of the moon what you would see if you went there?

It is arguable that an unperformable operation has value in stating the conditions by which a construct could be validated. Such a statement shows that the construct is not at the moment valid.

(b) Is there a use for hypothetical operations that would define constructs which are actually at the moment non-existent?

Red and green are supposed to be derived from yellow in the course of evolution. The discriminatory operations which would establish the existence of

two new colors, derived similarly from blue, could be stated, although they could not be performed at the present stage of evolutionary development. The operations which would define a new invisible planet are similar.

(c) Is there a use for hypothetical operations which could never be performed?

The definition of infinity depends on operations which can never be completed.

4. Is *experience* a proper construct for operational definition?

It has been held that experience is ultimate, subject to immediate intuition but not to operational definition.

5. Are there scientifically good and bad operations, and how are operations evaluated if they differ in value?

Objectivists hold that the data of experience can always be operationally defined if the data become public, because the operations of publication define the datum. It is, however, argued further that the operations of verbal report are 'poorer' than the operations of discriminatory choice (C.R.; jumping stand) because the verbal response itself involves terms that are less rigorously defined.

6. Is operationism more than a renewed and refined emphasis upon the experimental method (as understood already by Galileo, if not even by Archimedes)—i.e., a formulation of modern scientific empiricism and pragmatism (especially of the Peirce-Dewey variety), mainly of criteria of factual meaningfulness and empirical validity?

7. Must operationists in psychology relegate theorizing of all sorts to the limbo of metaphysics? Bridgman in physics is perfectly aware of the value of theories as long as they are in keeping with his operational requirements. The Gestaltists, particularly Köhler and Koffka, have repeatedly attacked posi-

tivism (an identical twin of operationism), reproaching it for its (alleged) opposition to theoretical construction. C. C. Pratt (*Logic of Modern Psychology*, pp. 147-154) on the basis of his operationism maintains that all theoretical explanation is circular or tautological. Köhler (*Dynamics in Psychology*, pp. 107-125) holds a strictly opposite view. Which position is the most adequate for psychological research?

8. Some radical operationists assert that the meaning of a quantitative concept lies exclusively in the set of measuring operations which determine the application of the concept. (E.g.: "Intelligence is what the intelligence test tests.") But how can we then know what it is that we are after in constructing tests; and what possible meaning is there in talking about improving or revising tests and measurements if there are no criteria outside the chosen test methods?

9. Are *all* scientifically legitimate definitions operational in character? This is (at least in part) a terminological question, but certainly one that it would pay to settle (not only) among psychologists.

10. What is a definition, operational or otherwise? It is important to know whether one is presupposing a logical apparatus for dealing with the language of science or intending through a psychological analysis to justify such an apparatus.

11. For the purpose of operational definition, what class or classes of events may be used properly as defining-operations? Specifically, can a phenomenon be identified or its properties be defined in terms of the events (operations) which are effective to produce, or occur as results of, the phenomenon?

THE EDITOR

THE USE OF OPERATIONAL DEFINITIONS IN SCIENCE

BY EDWIN G. BORING

Harvard University

Question 1 (a). The primary advantage of operational definitions lies in the unification of science and the resolution of controversy. The purpose of science is the simplification of our knowledge of nature under a set of broad generalizations, and the simplification is greatest when laws are stated in a single language, which inevitably turns out to be 'physicalistic.' Scientific controversy seldom involves disagreement about the observed primary data. It occurs usually in connection with the interpretation of the data, arising on the occasion of the validation of concepts or because of the ambiguity of meaning of conceptual entities. Positivistic procedures force such concepts and entities back to their observational bases and thus out of the realm of disagreement.

Question 1 (b). When a speaker's statement is not clear to a hearer, it is always fair for the hearer to ask the speaker for an operational definition of the terms he has used. Since the operational definition introduces new conceptual terms, a regress is begun, one which may be infinite. This regress is not, however, embarrassing, for it can be terminated as soon as there is mutual understanding between the speaker and the hearer, as soon as there is no further demand for definition. Such closure may, however, always be opened again when a new hearer enters the discussion.

Question 2. A concept like *distance* requires operational definition, but it is possible to identify two operations in terms of further operations. There are operations which establish the equivalence of triangulated and tape-measured distances when they are of terrestrial magnitudes, so that one can for short

distances speak of *distance* and mean either or both operations. Such equivalences are, however, valid only within the probable error and limits of the operations involved, and are subject to the dubiety of interpolation, extrapolation and inferential inadequacy. Nevertheless, it is only by such processes that one passes from the particulars of specific operations to the most useful larger generalizations.

Question 3 (a). Bridgman has shown that the concept of absolute quality for hue is a pseudoproblem, since one hue can be defined only in its relations to others. It is, however, possible to define the principal hues relativistically by discrimination. Principal green is the hue that is greener than any other hue, greener than any yellow-green, greener than any blue-green. One could conceivably find out whether certain animals perceive principal hues and how many. It is not certain, however, whether these techniques of discrimination are practicable with animals. They have never been developed. Still principal hues for an animal can be given such an operational meaning. They are defined by the system of discriminations which would be found if such experiments proved practicable.

Question 3 (b). Similarly there is statable an operational definition for principal hues in respect of an animal that turns out eventually not to perceive principal hues—unattainable operations which define a non-existent entity. The operations can also be laid down that would define the next pairs of hues to develop in human evolution, the pair that would have the relation to blue that red and green have to yellow.

Question 3 (c). Such statements of hypothetical operations tend to clarify the understanding of analogous operations that can be performed with positive results and also to suggest research.

Question 4. *Experience* and all the 'subjective' entities are as much in need of operational definition as are any other scientific concepts, or are more so because of their equivocality. To say that *experience* reduces operationally to *discrimination*, meaning that discrimination is itself differential reaction, statable, when not private, in physical terms, is to bring experience within the universe of scientific experimentation and psychology within the unity of the sciences.

Question 5. Operations can be good or poor. Operations that bring scientific concepts into relations with larger systems of generalization tend toward the unification of science, are the most productive definitions, and as such are better than more isolated operations. Thus operations that depend on the distinction between the meaning of the words *verbal* and *non-verbal* are poor, because one wants at once the definition of a *word*. If one defines a word as a symbolic or vicarious entity to which a subject reacts as he would to the equivalent referent which the word symbolizes, then one has a better, less mystic definition, even though it then be found that animals and even inanimate objects 'understand words.' The test of goodness of an operation is its univocality.

Question 6. All positivism from Mach to Bridgman has stressed the advantage for clarity of reducing dubious concepts to empirical ultimates—either to the data of observation (Mach) or to the processes of observation (Bridgman). Operationism is, however, something more than experimentation because it aims, not at the pluralism of particular experiments, but at the unification of science in the most general system of terms available. The effort to lay down op-

erational definitions has the effect of influencing experimentation toward the attack upon the more significant problems.

Question 7. Every scientific datum is public. Science does not consider private data. If a datum is public, the operation of its publication is statable. There can be no scientific data that can not be operationally defined. On the other hand, the meeting of the demand for clarification by operational definition takes time and thought. Not all scientists wish to spend their energies in this way. Gestalt psychologists prefer to depend on the adequacy of a free use of language to establish satisfactory communication. They should not be coerced into greater precision of language. Let the operationists state their operations for them or puzzle them out, while the Gestaltists are left free to discover what their trust in less rigorous language will achieve scientifically.

There can, of course, be private operational definitions. A deaf and dumb man on an uninhabited atoll might make them and use them. They would still be likely to have the form of self-communication, but their terms might not always be 'physicalistic.'

Question 8. Operationism is not opposed to the validation or extension of a concept. If intelligence is what the tests test, it is still possible to ask whether what the tests test is neural speed or normal education or something else. If it should turn out to be normal education, then you have two equated operations for the same entity. (See my answer to question 9.)

Question 9. Since science is empirical and excludes private data, all of its concepts must be capable of operational definition. *Mental energy* is an example of a concept that usually lacks operational definition, although of course it could be given one in each context in which the term is used. *General ability*

has a clear operational definition in terms of the correlational matrix from which it is derived. The *supernormal phenomena* of psychic research have many particular operational definitions, but the general definition is poor because it is negative, because it defines *supernormal* as ignorance of the relation of the phenomenon to other known systems.

Question 10. A definition is a statement of equivalence. A term is defined when it is pronounced the equivalent of other terms or events. Not all definitions are operational, but the most rigorous definitions are. Non-operational definitions are apt to be statements of synonyms. See any dictionary.

Question 11. A phenomenon can be identified in terms of its antecedent or consequent phenomena if the correlation is symmetrical and perfect. No operational definition inheres, however, in the

mere statement of such equivalence, but the statement can not be made rigorously unless the operational definitions of both terms are statable. If they can be stated and the correlation is found to exist, then the equivalence of the two operations is established. (See my answer to question 2 supra.) Is *hearing a tone* equivalent to *excitation of the temporal cortex*? You need the operations for hearing a tone and for excitation of the cortex and also the observation to establish the equivalence of the two. On the other hand, is the *super-ego* equivalent to *conscience*? That is a matter of a priori definition, and one may choose to define *conscience* as *super-ego*, furnishing an operational definition of the *super-ego*. Only if one has an independent operational definition for *conscience* can the question of empirical correlation be raised or answered.

SOME GENERAL PRINCIPLES OF OPERATIONAL ANALYSIS

BY P. W. BRIDGMAN

Harvard University

The reader need hardly be warned that as regards specific applications of any of the following to psychology I have only the competence of an outsider.

I shall not attempt to answer the questions in order but shall rather give a more or less general exposition in which many of the topics touched in the questions naturally present themselves, but in a different order. Questions 9, 10 and 11 touch on fundamental matters which may be answered incidentally in a general discussion.

It will, I think, not be unduly restrictive to consider definitions as applied to terms. A term is defined when the conditions are stated under which I may use the term and when I may infer from the use of the term by my neighbor that the same conditions prevailed. Any method of describing the conditions is permissible which leads to a characterization precise enough for the purpose in hand, making possible the recovery of the conditions to the necessary degree of approximation. Terms used in a scientific context must be subject to the presuppositions of scientific enterprise. One of the most important of these is the possibility of checking or verifying the correctness of any statement; in fact, this may be taken as characteristic of *any* serious enterprise. Checking that the conditions are satisfied is done by performing certain operations, so that for all essential purposes the definition may be specified in terms of the checking operations. These operations usually involve some element of deliberate direction by the checker; this deliberate direction may not be any more articulate than placing one's self in such a position that one's sense organs may be acted on

and in paying attention to the resulting sensations. In any event the process of checking involves activity of some sort, so that from the point of view of utmost generality, in which the operation is understood in the sense of any conscious activity, definitions *must* be operational and it is tautological to speak of operational definitions. However, such great generality is of little use. In order to be of practical value, the operations must, at the very minimum, be such that they are repeatable and performable on demand; in fact, this would seem implied in the idea of checking. Whether there are such operations or not can be found only by experiment, and, as in every experimental situation, absolute precision is not attainable. No operation can be specified with absolute precision, and no attempt to repeat an operation can be proved to have been completely successful. But in practise there are operations which can be repeated by the same person or different persons under the same or different conditions without hesitation and with the accompaniment of no phenomena which demand the assertion that there has been failure to repeat. Definitions should be framed in terms of operations of this sort. It is not necessary that such operations be especially simple, or that they be analyzed into all discernible components—merely that they be repeatable with assurance.

One of the greatest advantages of an operational break down of a situation is that it reduces it to a description of an actual happening—of something that has actually been done or that has actually occurred—and therefore it has the validity of actual experience. In particular, there is no room for concealed con-

traditions. "Hypothetical operations" (Question 3) may be of doubtless value, but the full operational meaning of 'hypothetical' is complex. A hypothetical operation must have its meaning in terms of a program of actual operations, into which it can be broken down. It is to be regarded as a shorthand statement indicating that if certain operations are performed and certain results obtained, then certain other activities will be engaged in, but the program, if valid or even meaningful, must be capable of being broken down into operations which we now know how to perform. The meaning of "the other side of the moon" (Question 3, [a]) is complex and indirect, but expressible in terms of presently performable operations, such as observing the change in the illumination of Mars when the other side of the moon shines on it.

It is my personal opinion that the paradoxes of the infinite in mathematics (Question 3, [c]) arise precisely because in mathematics infinity is sometimes defined in terms of impossible operations, and that in all cases where the concept of infinity is legitimately handled its definition can be reframed in terms of actual operations.

One of the chief advantages of making definitions operational is that it increases precision (Question 1). I find that I myself use terms without either precisely formulating the conditions which demand their use, or without, in certain cases, having more than a vague feeling that the conditions are now such that the term may be appropriately used. When my neighbor uses the term, I observe only too frequently that I cannot deduce from his use of it the occurrence of certain antecedent or subsequent occurrences which I believe are implied in his use of it. In such cases reduction of definitions to operational terms increases precision, so that operational definitions are called for when increased precision

is called for. Furthermore, by exposing the nature of the underlying operations, we discover whether our terms are really good for what we thought they were good for. To do this we capitalize any experience we may have had. In this way we often find that we have been mistaken in our uncritical opinion of what went into the term. This procedure is especially valuable in exposing verbalisms.

A strict application of these ideas means that one never has the 'same' construct defined by two independent operations (Question 2), but in this case there are properly two constructs which may be proved by experiment to give results indistinguishable within certain margins of experimental error and within certain ranges of phenomena. But it is never safe to assume that they will continue to be equivalent in a new range as yet unexplored. In fact, this is one of the advantages of the operational method; it ensures against a repetition of the situation in physics which led to the adoption of the theory of relativity. This situation arose from the uncritical assumption of the equivalence of operations at high velocities which had been shown to be equivalent within experimental error at low velocities.

Whether 'experience' is a proper construct for operational definition (Question 4) is a question for experiment. One has to ask in the first place whether a definition is necessary in the sense that misunderstandings arise because of its usage in different ways by different people. One then has to ask whether an operational break-down into simpler components is possible, and this question has to be answered by experiment, which in general will involve actually producing the simpler components.

Operational definition certainly may and often does form a regress (Question 1 [b]), but I can see no reason for thinking that it may be an infinite re-

✓ gress. It seems to me that the situation may be more like the situation presented in mathematics by semi-convergent series, in which the goodness of the approximation can be improved up to a certain point, but beyond that divergence begins. The specification of operations within operations I believe may presently pass beyond the range of verbal language and reach a situation where one can only point at an operation and imitate it. The attainment of further precision on this level must be difficult to say the least, and we may find ourselves stymied in trying to make indefinite progress.

Since operations are usually used in a context of purpose of some sort, one may if one likes speak of good or bad operations (Question 5), but I personally would usually prefer to speak of useful or non-useful operations. It certainly must be recognized that some operations are more useful than others.

Operational analysis is mostly restricted to questions of meaning, and as such can have only partial congruence with the universe of experimental method (Question 6).

✓ I can see no reason why the operational method should have any inhibiting effect on any legitimate theorizing (Question 7), and in so far as it has any effect at all, it can be only beneficial because it increases precision. Pratt's objection that all theoretical explanation is circular amounts to an argument that induction is circular, which I believe to be true and to be particularly obvious in the light of an operational analysis of what is involved in an induction. The fact of circularity need in no wise detract from usefulness.

Part of Question 11, "Can a phenomenon be identified or its properties be defined in terms of the events (operations) which are effective to produce it?" is closely related to the dictum of Question 8 that "intelligence is what the in-

telligence test tests." Without doubt it is possible to apply the procedure suggested here, but I believe that the situation seldom arises which one would be content to treat finally by any such method as this. Such procedures should be used only in the preliminary stages while the phenomena are still incompletely explored and many correlations remain to be found. In fact, it seems to me at least debatable whether even in the preliminary stages other procedures might not sometimes be better. "To define a phenomenon by the operations which produced it" involves unproved assumptions. It implies that performance of the same operations will always be followed by the occurrence of the same phenomenon, and this statement is operationally meaningless unless there is some method of checking the truth of the statement. This again implies that it means something to say 'same' phenomenon, which implies, unless we are here dealing with a pure convention, that there is some other method of recognizing the phenomenon when it recurs than through the operations of the definition. Operational definitions, in spite of their precision, are in application without significance unless the situations to which they are applied are sufficiently developed so that at least two methods are known of getting to the terminus. In the situation above, definition of a phenomenon by the operations which produced it, taken naked and without further qualification, has an entirely specious precision, because it is a description of a single isolated event, without even the existence of a criterion to determine when it recurs or whether the description of the event is complete.

✓ With regard to the intelligence test, the assertion as it stands begs the question. The question-begging word is the humble 'what.' The assertion that the intelligence test tests a 'what' implies

the repeated application of the test and the discovery that the results of the test have the properties of a 'what.' It seems to me that the actual situation here is one of spiral approximation, as it so often is. We use the word intelligence to describe certain aspects of the behavior of ourselves or our fellows, but with undesirable vagueness and subject to conditions which we do not fully realize ourselves, and which we discover only by experimenting with ourselves by asking whether we would call such and such concrete behaviors examples of intelligence or not. On the other hand, we have the practical problem of discovering some simple procedure or test which, when it is applied and gives certain results, will give the same answer as the more vague procedure which we often cannot formulate in advance. Our attempts to do this are at first unsuccessful. We discover modifications in the test which give greater success, but at the same time, by checking in a large number of instances we refine our original conception of what we will be willing to call intelligence, or may even discover that what we were trying to do cannot be carried through. But if it can

be carried through we have come out with at least two different methods of getting to the same place, one of which is presumably so much easier to apply than the other that we shall use it when feasible.

With regard to the analysis of experience into discriminatory responses, it seems to me that this must be only a preliminary stage which must eventually be split into simpler components. The complete operational specification of the antecedents of the response includes an operational description of the subject of the test, and since the directions for the test at present have to be given linguistically, all the questions of language and complete cultural background of the subject are involved. We should eventually be able to answer such questions as to what extent the discriminatory response is conditioned by the cultural background of the subject, and this cannot be done, or at any rate has not been done, with present methods of procedure. The question becomes especially pressing in connection with such discoveries as, for instance, the ability to discriminate a 'volume' characteristic of sound distinct from its physical intensity.

OPERATIONISM AND SCIENTIFIC METHOD

BY HERBERT FEIGL

University of Minnesota

Question No. 6. 'Operationism' is a new name for certain fairly generally recognized aspects of scientific method. It is indeed no more than a refined and modernized emphasis upon the requirements that scientific concepts must meet if they are to be meaningful and fruitful. Descriptions, laws, hypotheses, and theories may be critically examined on the basis of such criteria applied to the concepts they contain. In the perspective of the history of science and the history of philosophy, operationism represents a recent formulation of some of the essential features of the experimental method and of empiricism generally, accentuated in the direction of pragmatism and instrumentalism (Peirce, James, and Dewey). Bridgman's formulations of the criteria of empirical meaning, though probably quite original with him, have much in common especially with C. S. Peirce's in "How to make our ideas clear" (first published in 1878). There are two questions with which we are (or at least *should* be) concerned in any cognitive enterprise: "*What do we mean by the words or symbols we use?*" and "*How do we know that what we assert in these terms is true (or confirmed to some degree)?*" The characteristically pragmatic turn becomes manifest when these two questions are scrutinized in the light of two further questions, "What do we *do* in order to find out whether a term is legitimately applicable?" and "What are the *fruits by which we shall know* whether the introduction of a term is scientifically useful?" The first of these two latter questions stresses the element of active intervention in measurement and experiment. The second intends to remind us that concepts are

scientifically worthwhile only if they help in the task of prediction (and, possibly, practical control). The essence of the operationists' emphasis may thus be seen in the formulation of criteria of scientific meaningfulness and fruitfulness for concepts and of criteria of validity for factual statements. While these criteria were implicitly respected in much of the scientific work of recent centuries (and even occasionally to some degree explicitly formulated), the trend of our age toward a fuller awareness of and critical reflection upon the underlying presuppositions and guiding principles of our thinking and doing has, among other results, promoted a more penetrating analysis of scientific method. The particular interest in operationism may be understood as arising from the need (1) of purifying scientific method by the elimination of pre-scientific and non-scientific (e.g., metaphysical) elements, and (2) of understanding more clearly the meaning of the highly complex concepts employed in the more abstract and constructive levels of modern scientific theories. Such critical awareness is especially valuable when one needs to examine science where it is in the making. The numerous approaches in recent psychological theory are most certainly a case in point.

Questions 10, 1, and 9. Clarification or analysis of meaning is pursued by *definition*. Definition, i.e., the specification or delimitation of the meaning of a term or symbol, may be considered a statement of the *rule concerning the use* of a term or symbol. The need for definition practically arises only when (a) we are not sure what a given term or symbol means, if it means anything at

all (obscurity); (b) where there is a plurality of meanings (ambiguity)—to be removed by multiple definition; (c) when the term or symbol in question is used so vaguely that a definition giving it greater precision is demanded; (d) when the term or symbol, though defined clearly, unambiguously, and precisely in some respects, is to be given a place in a wider context and thereby enriched in meaning. This wider context may be a system of symbols only (such as we find in a pure calculus) or it may include symbols with empirical reference—in this latter case the definition may be 'coördinating,' *i.e.*, relating a symbol of an abstract system to terms which through previous definition already possess empirical reference; (e) finally, definition is needed whenever a situation in research calls for the coining of a new term as an abbreviatory convenience for more complex aggregations of terms either already in use or logico-mathematically so aggregated for the first time. This latter condition arises particularly in the context of the discovery of new elements or relationships in the subject matter of research.

Definition as here conceived is *nominal* (*i.e.*, definition of *terms* or *symbols*). So-called *real* definitions (of *things*, *properties*, etc.) reveal themselves either as empirical descriptions with all terms understood (by previous nominal definition) or as characterizations of things for the sake of identification. Since, in this latter alternative, identification occurs through labelling of things on the basis of observational test it is tantamount to *nominal* definition of the label. Just how precisely, completely or directly operational definitions enable us to identify objects is a matter of great methodological importance, but also surely a matter of degree.

To demand definition of *every* term used in a piece of scientific discourse would not only be unduly pedantic (beside being incapable of practical fulfil-

ment and thus utopian) but also quite unnecessary. The adult and sane use of common language is on the whole sufficiently definite to permit intersubjective communication and intelligibility as regards terms representing things and their observable properties. Doubt as to meaningfulness or as to precise meaning arises usually only regarding higher-order constructs and/or terms of fairly clear subjective meaning but lacking sufficient determination for successful intersubjective testability. Doubts of this second sort are rare in physics but plentiful in psychology. Doubts of the first sort may arise in any science which organizes its subject-matter by means of constructs above the level of observable-property predicates.

Aside from these practical considerations, it is obvious also from a logical or epistemological point of view that there can never be an occasion for an unlimited regress in definition. A series of definitional steps may be long, but it will terminate with definientia which are linked to something outside the realm of terms and symbols—namely, items of direct observation. As to whether this last step in the definition of any empirical concept is to be considered itself a kind of definition is a mere question of terminology. It is rather fashionable nowadays to speak of '*ostensive definitions*.' Psychologists in particular should not have much trouble with this: our use of language is after all a product of learning and we learn the intra-linguistic relations of words to one another as well as the extra-linguistic application of *some* words to items of experience. What from the viewpoint of logical analysis appears as a rule for the use of symbols represents itself as a habit (or rule-regulated pattern) of symbolic (*i.e.*, verbal) behavior to the psychologist. An ostensive definition, then, may be considered either as a designation rule formulated in a seman-

tical metalanguage or as a piece of practical drill in the learning of the 'right use' of words. Quite generally, an exact logical analysis of the meaning of scientific terms requires the use of the apparatus of syntax and semantics. The corresponding psychological analysis can be carried out in the object language of (preferably a behavioristic) psychology, where words in use are described as physical events, *e.g.*, of emission of sounds, and are thus included in a general study of the behavior of human organisms. These two ways of studying definitions, and the uses of language generally, supplement each other very well, are entirely compatible, and fulfill, each in its way, important functions. The logical analysis examines given uses of language in the light of critical standards, such as consistency, non-circularity, sufficiency, etc. The psychological study is essentially a description and causal analysis of verbal behavior. Each can be made to reflect the other on any level of investigation. The psychologist in his study of definitional behavior uses, at least implicitly, definitions himself—and thus provides subject-matter for logical appraisal. And the logician in appraising definitions (or in any other syntactical or semantical pursuit) is, after all, *behaving* in specifiable ways and thus furnishes material for the psychologist.

If all definitions amount to rules delimiting the use of terms or symbols, aren't *all* definitions operational in character? This is indeed a purely terminological question. But terminology is a significant weapon in the strategy of scientific enlightenment. It is my personal conviction that the battle-cry of operationism can have its intended beneficial effects only if the meaning of 'operational' is confined in its application to the definition of *empirical* concepts. The problems of concept formation in pure logic and pure mathematics are of

a very distinct character, and rash transference, by analogy with the empirical sciences, of conditions for restrictive meaning is, to say the least, of highly dubious value. When we demand operational definability for the terms of the factual sciences, we may (and must!) indeed include purely calculational operations—but these operations, called variously 'mental,' 'paper-and-pencil,' or better 'logico-mathematical,' should be applied to root-terms which have *empirical* reference. Without this restriction the term 'operational' would become synonymous with, say, 'functional,' implying that any definition that is worth its salt must specify the functions that the term to be defined is to fulfill. Such 'functional' definitions could be given for the terms of pure mathematics on the one side but, on the other hand, also for terms used in the most transcendent speculations of theology and metaphysics. As Bridgman has repeatedly stated, the original impetus toward his operational analysis in physics came from a consideration of Einstein's procedure in the theory of relativity. Einstein had realized that certain terms, like 'absolute length,' 'absolute duration,' 'absolute simultaneity,' in Newtonian physics were devoid of empirical meaning because no observational or experimental procedures were or could be specified for their application. The operational criterion here serves to distinguish physics from metaphysics. It will seem obvious to many psychologists that, for example, Freud's 'death instinct,' though possibly of some emotive or literary value, is devoid of factual, scientific meaning. To put it briefly, if crudely, operational analysis is to enable us to decide whether a given term, in the way it is used, has a 'cash value,' *i.e.*, factual reference. If it does have factual reference, operational analysis is to show us *precisely what* that

factual reference is, in terms, ultimately, of the data of direct observation.

Question 3. The velocity of a uniformly moving vehicle may be determined by means of a yardstick and a stopwatch. The I.Q. of a person may be determined by the Binet-Stanford testing procedure. In both cases a simple arithmetical division (s/t ; mental age/chronological age) yields the final result. In the case of more complex concepts or higher-order constructs, the mathematical operations by means of which these constructs are defined are correspondingly more complicated. The factual reference of scientific concepts in physics or psychology, however, depends not so much upon the purely mathematical operations but rather upon the observational and manipulatory (mensurational, experimental) operations. These establish the link between the empirical (or descriptive) terms of our scientific language and the data of experience. Again, it is a merely terminological question as to whether simple acts of perceptual discrimination and identification by themselves should be labeled 'operations.' Such acts are certainly involved in the terminal ostensive steps of any definition of factual terms. Most of the cases in which definitions are called for, present situations of greater complexity. Thus the question concerning the *possibility* of operations practically always amounts to asking whether certain measuring or testing procedures referred to in the definition of a term can be carried out. But 'possible' and 'can be' are notoriously ambiguous words. We must distinguish between (a) logical ('in principle'), (b) empirical (natural), and (c) practical (technical) possibility. Since the limits of practical possibility are relative and, as a matter of fact, receding with every advance in the techniques of observation and experiment, no scientist restricts factual meaningfulness to testability

within the bounds of the technical facilities of the moment. Bigger and better microscopes, telescopes, electroencephalographic instruments, etc., are obvious reminders. Of course, there is always an element of risk involved if definitions of terms hinge upon operations that are technically not feasible at the moment. It is like issuing or accepting a promissory note. But, if the operations are compatible with well-established natural laws, merely practical impossibility of the testing procedure does not deprive the term in question of its scientific meaningfulness. All we may have to wonder about is the fruitfulness of concepts whose definition necessitates reference to operations too far removed from technical feasibility at the time. Concepts of atomic structure were very far beyond practical testability only forty years ago. Concepts regarding the nature of cerebral memory traces are in the promissory-note stage today. Scientific research as an ongoing process involves a continuous scale of degrees of technical testability. Considerations of a very practical and inductive sort only enable the scientist to draw a line between operationally satisfactorily and unsatisfactorily defined terms. Much more serious is the limitation of empirical (or natural) testability. A testing procedure that is incompatible with well-established natural laws will never define a scientifically acceptable concept. For example, the very well confirmed laws of quantum mechanics exclude operations which would enable simultaneous determination of the speed and the location of electrons. Therefore it is now generally agreed that some of the customary concepts of classical mechanics as applied to electrons are not false but scientifically meaningless. Absolutely or downright meaningless (*i.e.*, devoid of factual reference) are terms whose definition would involve contradictions with any testing procedure.

Vitalistic conceptions of 'entelechies,' for example, belong in this last category (of by definition unconfirmable ideas).

Question 7. Operationism has occasionally hypertrophied into a radically anti-theoretical attitude. Ever since Galileo replaced the question "Why?" by the question "How?" and since Newton pronounced his (much misunderstood) "Hypotheses non fingo" positivistic scientists have been inclined to restrict their endeavors to pure description and correlation. *Explanation* is considered a metaphysical misfit. Mach and Ostwald rejected the atomic theory in physics and chemistry. Some outstanding psychologists of our day still reject as non-operational the psycho-analytic theory of the unconscious; others consider neuro-physiological hypotheses when used as explanations for behavior as so much metaphysical verbiage. If the student of the history and the methodology of science be permitted to mediate in this quarrel, he would say that, as so often, the truth lies somewhere in the region of the golden mean. The issue is closely connected with the one discussed in the preceding section. The positivists are objecting to the use of terms (and theories employing such terms) which are only very tenuously and indirectly connected with the evidence technically accessible at the moment. They are, temperamentally, perhaps because of a general fear of the intangible, extremely wary and reluctant in accepting promissory notes. But their lack in confidence is more than made up by the enthusiasm of the theorists. And, just as in other matters involving guesswork, it is hard to tell whose attitude will prove more fruitful in a given case. In physics the atomists won out over the positivists. The ether-theorists lost. In psychology it is perhaps still too early to tell, but it looks as if substantial components of psycho-analysis as well as of neuro-physiological be-

havior-theory were to stay with us and to be developed more extensively. The case of psycho-analysis is particularly favorable if some of its outright mythological and metaphysical features are dismissed and the remainder translated into behavioristic terms. Such systems as those of Hull, Tolman and Skinner are generally quite in keeping with the narrower operational criterion of meaning. Relatively few references are made to operations of a purely hypothetical sort. The essential point worth noting is that even in these highly positivistic approaches *explanation* does have a legitimate place. On the basis of only indirectly confirmed theoretical assumptions (not only of experimental laws) more specific descriptions of phenomena are logically deduced. The difference between these approaches and the theories condemned as speculative seems to this impartial observer one of degree. W. Köhler, in his *Dynamics in psychology*, may be said to issue a great number of promissory notes. But who can tell how soon they may be backed by independent verifications coming from neuro-physiology? His critique of C. C. Pratt's conception of scientific explanation and the function of theories seems perfectly adequate. All one can say in favor of the conservative side is that the knowledge of empirical laws concerning behavior in terms of macro-concepts is still far from being complete and that a long and fruitful period of investigations on this level of analysis will yield significant results—which will in any case be required in order to test neuro-physiological 'micro'-theories.

Questions 8, 2, and 11. If the linear expansion of a mercury column in a glass tube of even width furnishes the basis of an operational definition of 'temperature,' then the question whether mercury expands in linear proportion to temperature (so defined) must be answered with "yes" as a matter of logical

necessity or tautology. Analogous considerations apply to similar questions regarding psychological concepts (such as I.Q., habit strength, excitatory potential, etc.) operationally defined by standard testing procedures. Out-and-out operationists have often resorted to such easy and dogmatic quips as: "Intelligence is what intelligence tests test"; "temperature is what thermometers measure." Statements of this sort may be intended to intimidate inquirers of a somewhat mystical or metaphysical bent. But they do not even begin to give an adequate account of the meaning of concept formation in the sciences. Quantitative concepts of the type just mentioned are usually the product of long labors of adjustment by repeated redefinition. Thermometers or intelligence tests did not arise in an historical vacuum. They were devised in a context of problems that arose out of a background of previous qualitative and semi-quantitative knowledge. Problems of description and prediction led to a search for suitably precise, objective, and fruitful concepts. The concepts thus designed, in their logico-mathematical (functional) relations with other concepts, were to represent *empirical laws*, i.e., relationships between the various measurable (or at least testable) variables (or factors). In the light of these considerations it makes perfectly good sense to ask whether a mercury thermometer measures temperatures adequately. There are empirical laws, such as the First Law of Thermodynamics, which relates temperature to mechanical energy. If we wish to give this law its most universal and simple form, we are forced to consider thermometer readings of *any* sort merely as an approximation which is to be corrected or replaced by the Kelvin scale. Similar but usually more complex considerations apply to psychological magnitudes.

Empirical laws enable us to define the

same concept by different operational routes. The fact that length may be defined by the yard-stick as well as by the triangulation techniques is a consequence of the empirical laws of (applied) geometry according to which hard and solid bodies as well as light rays exhibit (in terrestrial experiments with a high degree of accuracy) Euclidean relationships. Nevertheless, Bridgman's warning to the effect that different operational routes define *different* concepts is not unjustified: The convergence of operational results is to be taken for granted only until further notice, i.e., until evidence to the contrary emerges. But until then, as a policy of typically inductive procedure and with all the provisos just made, it is one of the most helpful devices of scientific method to *identify* the concepts corresponding to results of convergent operational routes. This becomes especially clear when we consider the role of scientific theories.

A *theory* may be regarded as a set of assumptions from which empirical laws are derivable by logico-mathematical deduction. From physical theory we can deduce the electrolytic, magnetic, and thermal effects of electric currents. From a theory of learning we can deduce regularities of the emission or omission of responses. Through the unifying procedure of theoretical explanation we 'understand' what on the level of empirical law is a mere brute fact of functional dependency or correlation. Such 'understanding' is, of course, bought at the price of assumptions which are under the jurisdiction of confirming or disconfirming evidence. Deduction merely explicates what is implicit in the premisses. The theoretical constructs therefore contain in compressed form the empirical concepts which can be extracted by explicit definition.

Theories and the constructs which constitute them may, significantly, even if not too sharply, be grouped in two

classes. In physics it is customary to distinguish phenomenological from atomistic theories. Neither characterization is terminologically fortunate. We shall here call them quite neutrally 'theories of the first kind' and 'theories of the second kind' and distinguish them by illustrations. Chemistry with its concepts of elements, compounds, compounding weights, affinities, etc., is a theory of the first kind. So is classical thermodynamics with its concepts of energy, thermodynamic potential, and entropy. Atomic theory and statistical thermodynamics are theories of the second kind. In psychology such theories as those of Hull, Tolman, Skinner, and Lewin are of the first kind. Neurophysiological theories such as those advanced by Sherrington, Adrian, and Köhler are of the second kind. The constructs of the theories of the first kind are homogeneous with the operationally defined terms in the empirical laws of the given fields. The constructs of the theories of the second kind are in this respect heterogeneous: Atomic structure, which explains chemical properties and reactions, is a construct mainly derived from spectroscopy. Concepts of nerve-currents and their patterns are distinctly physiological. If these physiological concepts are ever to furnish an explanation of behavior, they will have to be 'identified' with constructs of pure behavior theory. As a matter of program, but not of actual achievement, this is anticipated in Hull's system. What remains to be shown is the actual convergence of operational routes for both types of constructs. On the physiological side this is still in the promissory stage and may remain there generally still for a long time. This means that the operations for the definitions of constructs in theories of behavior of the second kind are largely hypothetical (*i.e.*, at present technically impossible). It will not do to consider them suffi-

ciently defined by the molar behavior route anymore than it would do to consider atomic structure sufficiently defined by operations available to chemists at Dalton's time. Yet, as pointed out before, these are matters of degree. The prototype of an operational definition such as we can advance for directly measurable magnitudes should not mislead us into banishing all concepts which do not come up to this high level of methodological aspiration. Even in physics we have to define many concepts (not only highly indirectly but) sometimes only very partially. Before any theory of x-rays was developed, x-rays were simply "what you got when cathode rays impinged upon metal surfaces," and "that which produced photographic images of a certain kind." Only as we advance in discovery and technique such very sketchy definitions are supplemented by fuller qualitative, quantitative, and far-flung relational characteristics. Operationism wisely understood and applied must take account and render account of the level of precision, completeness, and fruitfulness reached at the given stage of concept formation.

By way of an appendix to this section I should like to analyze by means of a simple schema the 'economic' function of concept construction. If we as yet have not reached the level of theory, we often prepare a place for theoretical constructs through the introduction of operationally defined empirical terms. The term 'electric current' had a sufficiently definite meaning already before the Maxwell electromagnetic theory or the electron theories had arisen. There were numerous different conditions which were said to produce electric current, and numerous types of effects which were said to be caused by the current. The current itself seemed to many an intangible mystery. Similarly, there are many ways of acquiring a habit. And there are many ways in which this habit manifests itself. But what is the habit? These foolish questions disappear

if one adopts the operationist outlook. The constructs ('electric current'; 'habit') are introduced in order to save statements. If there are m causal conditions and n possible effects we would need mn statements in order to formulate all possible observable relations. If, however, we introduce our auxiliary concepts the number of statements required shrinks to $m + n$. For large numbers m and n the conceptual economy is accordingly quite considerable. Since the established scientific procedures keep all constructs in principle open for additional definitional routes, m and n are never limited in a dogmatic manner. This feature in an enlightened operationism prevents the typical ultra-positivistic (I would call it 'negativistic') fallacy according to which things (particularly those inferred entities designated by constructs) 'are simply what they are known as' or 'are nothing but fictions introduced in order to speak more conveniently about certain sets of data.' Things are, rather, what they are known and knowable as; and the sets of data are in principle capable of unlimited extensions in various (sometimes even surprising) directions. Once the empirical constructs (*i.e.*, these auxiliary concepts) are linked to one another, either through empirical discovery or by deduction from theoretical assumptions of further functional relations between them, a whole network of variously connected concepts arises and the scientific discipline in question has attained a high degree of maturity.

Operational definitions then, or the concepts which they define, may be classified into various sorts: Purely qualitative; semi-quantitative ('comparative' or 'topological'—such as the hardness scale; or introspective concepts of 'more' or 'less'); fully quantitative or metrical (*e.g.*, temperature, loudness); causal-genetic concepts (*e.g.*, habit); theoretical constructs (*e.g.*, electric fields, excitatory potential). It depends entirely upon the level of research attained which of these forms or which of their possible combinations may be applied. Concept formation in psychology, and particularly in psychiatry, is on the whole still in a relatively unfinished stage. The situation is here similar to the one in medicine

where the definition of diseases is at first primarily in terms of symptoms, later in terms of more reliable causal-genetic data and, perhaps, finally in terms of a micro-account of the physiological type.

Questions 4 and 5. Since science is, as one might say, by definition, a social enterprise, it must insist upon operations which are repeatable not only by one observer but in principle performable by any properly equipped observer. A statement is scientifically meaningful only if it is intersubjectively testable. Subjective peculiarities, subjective differences can, of course, become the subject matter of scientific study—if—as in the psychology of individual differences—we can verify them in a perfectly objective manner. Private, immediate experience as such is only the raw material, not the real subject-matter of science. One's own immediate experience, the actual-lived-through stream of data, may therefore be conceived as the epistemological basis of all concept formation and theoretical construction in the empirical sciences. In that sense it is not a construct but that small foothold in reality that any observer must have in order to get at all started in his business of exploring the world of things and organisms surrounding him. If one wishes to convert this narrow realm of directly given experience into subject-matter for description one is limited to a purely phenomenological approach of the introspective type. The scientific value of such a study is very insignificant as long as it remains unrelated to a study of extra-dermal and intra-dermal stimuli (physical and physiological processes) or to behavior responses. If, however, one does so relate the phenomenally given to these processes inferred (or constructed conceptually), it can itself be conceptualized as overt or potential behavior (or by way of promissory hypothetical an-

ticipation as cerebral processes). That is precisely what happens in the "psychology of the other one," where one constructs on the basis of one's own data the other one's 'experience.'¹ And here again we have the choice of various conceptual systems or languages for description: mentalistic, behavioral, or—ultimately—physiological. In this sense, then, we may say that "the other one's experiences" (or better: the concepts describing it) are constructs. In this sense, also, and in this sense only, concepts referring to one's own experience are constructs. They designate processes which at least to a very small extent fall within the scope of direct phenomenal acquaintance.

Much as the epistemologist or phenomenologist may wish to clarify and formulate the meaning of 'experience' along these lines, there is the alternative of a strictly physicalistic or behavioristic approach right from the start. From the point of view of the methodology of science this is preferable, since it eliminates with one stroke the pitfalls of the traditional metaphysical pseudo-problems of solipsism, the mind-body puzzle, etc. If only intersubjective operations are admitted one's own experience is in every respect on a par with that of the other one. Introspection itself is then described as a response to previous responses ('early retrospection'). The only asymmetry that remains is that an individual person is able to predict some (but by no means all) of his own behavior better than another observer could. But that is a matter of degree. Many of the intra-dermal and particularly the cerebral conditions are not as

yet technically as directly accessible to outside observers as they are (however vaguely and diffusely) to the individual subject himself.

CONCLUSION

Operationism is not a system of philosophy. It is not a technique for the formation of concepts or theories. It will not by itself produce scientific results. These are brought about by the labor and ingenuity of the researchers. Operationism is, rather, a set of regulative or critical standards. In the light of these critical standards the meaningfulness and fruitfulness of scientific concepts may be appraised. It seems that the outstanding requirements which operationism has quite justifiably stressed may be formulated as follows: Concepts which are to be of value to the factual sciences must be definable by operations which are (1) logically consistent; (2) sufficiently definite (if possible, quantitatively precise); (3) empirically rooted, *i.e.*, by procedural and, finally, ostensive links with the observable; (4) naturally and, preferably, technically possible; (5) intersubjective and repeatable; (6) aimed at the creation of concepts which will function in laws or theories of greater predictiveness.

The degree to which these ideals are approximated varies from one science to another. But it would seem that all of these criteria are applicable not only to a well developed and systematized science such as physics but also to a science still largely in the making such as psychology.

¹ Operations which would enable one to 'inspect' the other one's private experience are by definition, *i.e.* logically, impossible. What would happen if nervous systems were connected is a matter of conjecture but certainly the logical impossibility of *having* the other one's experience will thereby not be removed.

NOTE: The epochmaking article by C. S. Peirce, 'How to make our ideas clear' appeared first in *Popular Science Monthly*, January, 1878. It was reprinted in *Collected Papers*, Volume 5; also in *The Philosophy of Peirce* (Ed. Buchler). For the relations of operationism to modern positivism, see my paper 'Logical empiricism' in *Twentieth Century*

Philosophy (Ed. Runes). The logic of empirical constructs is discussed in some technical detail by R. Carnap in 'Testability and meaning,' *Phil. Sci.*, 1936 and 1937. For a sketch of the same, more elementary, and applied to psychological concepts, see S. Koch, The logical character of the motivation concept, *PSYCHOL. REV.*, 1941, 48, 15-38; 127-154. The

operational criteria of measurement are very clearly formulated in G. Bergmann and K. W. Spence, The logic of psychophysical measurement, *PSYCHOL. REV.*, 1944, 57, 1-24. A very penetrating analysis of the epistemological problems of psychology has been given by H. Reichenbach in *Experience and prediction*, §§ 26, 27, 28.

TWO DIFFICULTIES IN OPERATIONAL THINKING

BY HAROLD E. ISRAEL

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Question 2. The operational technique originated as a corrective for a condition in physics in which a given construct such as length had come to have different meanings, different quantitative values, when measured by different methods (e.g., 'tactual' and 'optical') ordinarily accepted as equivalent. Bridgman located the source of error in the customary manner of defining constructs with reference to absolute properties conceived as transcending the methods used in determining their values. With a single, broad stroke he sought to eliminate the error at its source by removing from the definition and the meaning of constructs all reference to absolute properties in particular and all reference in general to anything outside of the actual, concrete operations performed in application. The original point of the operational technique, and its promise as a necessary reform in physics, consisted in the proposal to provide, in the form of operations, a single, tangible, ultimate base with reference to which all constructs could be unambiguously and finally defined.

According to the fundamental operational principle, a construct derives its meaning exclusively from a specific set of operations. "The concept of length involves as much as and nothing more than the set of operations by which length is determined."¹ If tape measurement is the method involved, then length involves nothing more than the set of operations performed in applying the measure. What can it mean, then, to say that triangulation constitutes an equivalent set of operations for meas-

uring length? From the operational point of view, the statement is either meaningless or is a direct denial of the basic operational principle. Tape measurement and triangulation refer obviously to two different sets of operations (the independence of the operations is not the point which concerns the operationist in this connection), and the related constructs defined purely with reference to the two different sets of operations must be said to be two really different constructs.

In what sense can two such different sets of operations be termed equivalent? The common answer given by the operationists themselves is to the effect that by operations of comparing the numerical results it can often be shown that two different sets of operations yield the same quantitative values in all cases considered, and that the two sets of operations are thus shown to be equivalent. This argument involves detaching the concept of quantitative value from its operational meaning and assigning to it the status of an absolute property, quantity of a kind which transcends the methods by which it is determined. Equivalence is a relative concept which demands a point of reference outside of the equivalent items; equivalence among operations demands a point of reference outside of the operations themselves. Operationally defined, two quantitative results can not be the same unless they are determined by the same set of operations, and the mistake of regarding two constructs as the same because they bear the same numerical designation is one which no operationist should make. By introducing the non-operational construct of absolute quantity the opera-

¹ P. W. Bridgman, *The logic of modern physics*. New York: Macmillan, 1927, p. 5.

tionist escapes from the narrow limits of his highly restrictive doctrine. He gains the freedom and convenience, perhaps necessary to sensible investigation and theory, of dealing with operations or methods as means, sometimes equivalent, of arriving at scientific ends. Is he not doing, however, the same thing which he caught the old physicists doing at the time he proposed the operational corrective, and is he not liable to make the old mistakes all over again?

Question 5. Instead of attempting to answer this question directly, it seems more profitable to raise a question which is preliminary and more fundamental: What is an operation?

In introducing his operational technique Bridgman seems to have anticipated no difficulty in the way of recognizing and identifying operations. He had a very definite conception of an operation as a unitary event, either physical or mental, whose nature could be so rigidly and uniquely specified that it could serve as the solid point of reference for the definition of all constructs. Actually, the technique of operational definition is concerned with sets of operations rather than with single operations, and a set of operations should, in principle, be rigidly and uniquely specified in order that it serve to define a specific, unambiguous construct. Rigid specification of a set of operations is accomplished in Bridgman's elementary illustrations by the direct method of supplying an exhaustive enumeration of component operations in the precise order in which they are performed.² This procedure is complicated, however, by the introduction of a second principle which holds that certain operations need not be mentioned when we know from experience that variations in these operations are without effect on the final result. At the outset, then, there seem

to be two conflicting conceptions of a set of operations. According to one, a set of operations is a rigidly invariable series of unique operational items, but according to the other, it is a purposive activity directed toward obtaining a certain result with the allowance of variation in the component operations.

Operational theory thus becomes involved immediately in the profound psychological problem of the nature of human behavior, or what constitutes a unit of behavior. In proposing his dual conception, Bridgman was probably unaware of the fact that in psychology the incompatibility of elementaristic and 'molar' conceptions of human activity has defined an issue so fundamental that it has given rise to directly opposed schools engaged in violent controversy. It seems fair to say that operational theory has been developed primarily upon the basis of a strictly elementaristic conception of sets of operations. It is even likely that molar views involving trains of operations directed toward ends are, in principle, incompatible with operational doctrine since variability or equivalence among member-operations raises difficulties in operational definition of the ends.

It is obvious that the apparently simple and direct procedure of defining constructs in terms of the actual, concrete operations involved is actually a psychological task so difficult that psychology in its present state is hardly prepared to handle it. The difficulty may be illustrated by considering one of the statements from the present question: "the operations of publication define the datum." What psychologist is prepared to issue a description of the operations of publication sufficiently specific and rigid to serve as the final, unambiguous definition of a given datum? And who is equipped to decide which of several possible descriptions is the more rigorous definition?

² *Ibid.*, pp. 9 ff.

OPERATIONISM IN PSYCHOLOGY

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Question 4. In reply to a critique (5) directed a few years ago against certain views which I had expressed regarding the nature of operationism, I confessed that it was temperamentally difficult for me to remain fixed and convinced about controversial matters in the field of psychological theorizing (8). The position which I tried to defend in 1939 represented the stronger convictions of Tuesday, Thursday, Saturday, and Sunday. If what I say now sounds more like what were at that time the weaker convictions of Monday, Wednesday, and Friday, I am not sure whether the change is due to an unstable temperament or to the pause which world events have given to the comfortable conceit that the speculations that were going on in the ivory tower before the surrounding lights went out either made much difference or had any importance.

At least on one matter no reason has been discovered for shifting ground. It is impossible for me to accept the view that *experience* is a proper construct for operational definition. A construct is something constructed. The initial data of science are not constructed. They are given. The world of nature, including human nature, is spread out for observation and inspection and constitutes the point of departure for every science. The different sciences represent accidents of history and limitations of interest, and are to be regarded today merely as more or less convenient although arbitrary divisions of labor. They do not represent basic differences in subject-matter, for the subject-matter of all sciences comes from the same initially undifferentiated stuff, *viz.*, direct experience. The difficulty with

trying to furnish an operational definition of immediate experience is not caused by the formation of an infinite regress. The difficulty or trouble is that any such definition is impossible.

Every scientific observation starts life as a bit of private experience. Someone has to report somehow what he observed somewhere. This statement is no less true of physics than it is of psychology. The star in the sky, as reported by an astronomer, is no more objective and public than is the hue of an after-image as reported by a psychologist. Whether any private experience can ever be the same for two people is a question to which there is no satisfactory answer. Whatever the answer, it has to be taken for granted, and on faith. The great majority of mankind, including scientists and even philosophers, have always taken it for granted that the world looks pretty much the same to all normal people. The reasonableness of this assumption is repeatedly confirmed by pragmatic test, *viz.*, in practice the assumption almost always works. The view that two people can have the same experience nevertheless remains an article of faith, and can no more be proved by operationism than it has ever been proved or disproved by any of the great arguments of philosophy. The escape from solipsism can only be made by a leap of animal faith. Most sciences have made the jump without much trouble, and often without knowing it. Only psychology stumbled and fell for reasons which are today fairly well known.

The separation of the natural sciences from philosophy was accomplished for the most part without any disputes over questions of ontology. The problem of

solipsism, if sensed at all by natural scientists, was left behind as a purely domestic quarrel among philosophers. The founders of modern psychology, on the other hand, fastened the problem of solipsism into the very center of their definitions. Once psychology was defined as experience dependent on an experiencing individual, the groundwork was laid for ontological polemics which have not yet wholly disappeared from the introductory chapters to psychology. The revolt of behaviorism only made matters worse, for in trying to pour out the polluted waters of mentalism, the behaviorist nearly lost the very infant he was striving to save. Mentalism is the starting point of all sciences, and is inescapably polluted by privacy. The initial data of behaviorism are no more public than are the data of introspection. Someone has to report the behavior of a rat in a maze. Someone has to report the appearance of red on a screen. Both reports intend experiences which are equally private. Their acceptance within the public domain of science is based either on pragmatism or faith, or both.

If the pragmatic test leads to doubt regarding the experience intended by any report, a situation which is happily not very common even in psychology, then it is necessary to state more precisely the conditions under which the report was made. If specification of these conditions does not lead to satisfactory duplication of the report, then indeed it may be assumed that something is wrong, and it is the traditional business of scientific method, or operationism, to set it right. The business of specifying the conditions which are capable of eliciting a reproducible report on a given experience may in certain experiments develop into an elaborate operational construction. The construction is the device which enables another person to go after the experience and find out

what it is like, in case he has any doubts. The construction and the experience are not, however, the same thing. Neither is the construction a definition of the experience. The experience is private and undefinable. It can only be pointed to. In difficult and elusive cases, the construction serves as a more accurate pointer.

Classical psychology, or introspectionism, after its long struggle with behaviorism, came to think of its initial data as peculiarly elusive and difficult of identification, although now that the dispute is practically over, it is puzzling to understand how anyone can any longer regard sensory experience, of all things in the world, as difficult to point to or identify. Be that as it may, the use of operationism as a pointer for those whose eyes have been blinded by the dust of a naive objectivism may serve a good purpose. The only danger is that the fervent apologists for neo-introspectionism, such as Professors Boring (1) and Stevens (10), will mistake the pointer for the thing pointed to.

The discriminatory response has become a well recognized operation for reporting on various kinds of experiences in psychological experiments. But to identify the response with the thing discriminated is to stir up more blinding dust than the radical behaviorists ever raised. "In thus placing discrimination at the basis of all science we allow it to usurp the position formerly enjoyed by 'experience' or the 'immediately given.' Does this substitution mean that experience and elementary reactions are equivalent? It does, precisely" (10, p. 95). There may be nothing in this position which a miracle of operationism can not cure, but if so, my eyes are still blinded. For if I understand the position correctly, it means that the color red and the stopping of a car at a traffic signal are one and the same thing. The arguments against this point of

view, and against the attempt in general to define experience operationally, appeal to me now even more strongly than they did when I directed some of them against Stevens a few years ago (7, pp. 102-106). The tremendous importance of sensory phenomena in military psychology ought to dispel any doubts whatever about their easy and effective translation from private to public property. It is an anachronism for psychology still to be bothered by the inescapable solipsism that enters into the problem. Operationism can not solve the problem. It can only show that the problem, being insoluble, belongs to metaphysics, not to science.

Question 7. One of the most inveterate characteristics of human thought is the tendency to assume that the events on nature's stage are controlled by forces behind the scenes. The main burden of Bridgman's operationism, as I understand it, is to show that science can neither prove nor disprove this assumption, for the assumption moves in a realm of discourse beyond the reach of scientific method. The only data with which science can operate are those which appear on the stage. What goes on behind the scenes can never be anything more than guesswork, and therefore does not belong in science.

Scientific explanation in the last analysis turns out to be a statement of the relations which have been discovered to obtain among observed events. "I believe," says Bridgman, "the essence of the explanatory process is such that we must be prepared to accept as an ultimate for our explanations the mere statement of a correlation between phenomena or situations with which we are sufficiently familiar" (2, p. 47). In the statement of a correlation, however, the language of science is closely snarled with the meanings and assumptions of prescientific common sense. If, for ex-

ample, the equation $F = ma$ is taken strictly, it must mean that force is equal to the observed relations summarized on the right-hand side of the equation. To say that the motion of an object is caused by force must therefore mean that the motion of an object is caused by the motion of an object, which is a tautology—a tautology, however, with a vast amount of predictive value in it. The reification of force into a thing which operates behind the scenes is a natural and almost inevitable turn of thought. In uncritical discourse the practice is ordinarily harmless, but if the reification is allowed to seep into the language of science, the infiltration may lead to undesirable deception regarding the extent and capacity of scientific explanation. My statement that science is a vast and impressive tautology (7, p. 154) has been called by Professor Köhler a discouraging thesis (3, p. 121). It would seem to me to be discouraging only to those who believe that scientific explanation can transcend the limits of observation.

Köhler's further strictures against what I tried to say about the circularity of scientific explanations are more weighty, and have already led to a partial revision of my thesis (8, p. 239). When the assertion is made that in the last analysis scientific explanation is a statement of observed correlations, it must of course be borne in mind that in actual practice the last analysis is only approximated, never finally reached. Further research turns up new items which, when added to the known and relevant correlations, make it possible and necessary to revise and improve earlier statements.

Correlations in psychology are for the most part obviously incomplete. The internal workings of the organism are vitally concerned in every psychological event, but since the precise correlation of these workings with behavior is still

obscure and in many cases unknown, the explanation of behavior remains tentative and subject to large revision. Psychology finds itself faced a good deal of the time with what Bridgman calls an explanatory crisis. How can this crisis be met? In psychology there are at least two schools of thought on this question. One flourishing point of view insists that all theorizing must confine itself to known correlations. Any attempt to make guesses beyond what is known leads to deception and useless circularity. Professor Lashley's attack on the synapse as a principle of explanation in learning is an example of this attitude. "If we deduce its properties [*i.e.*, those of the synapse] from the facts of learning, we gain nothing by explaining learning in terms of these hypothetical properties" (4, p. 473).

A good deal can be said in favor of this attitude. If conservatism can be defended in politics, it probably is entitled to some consideration in science. The majority of psychologists, however, seem to prefer a more liberal procedure in the construction of hypotheses. The difference in attitude is presumably to be ascribed more to temperament than to logic, and is also one of degree, not of kind—a fact which I am now inclined to think makes the difference between Köhler and myself in this matter more apparent than real.

Willingness to invent an hypothesis by drawing upon the unknown to account for the known almost inevitably involves circularity. The hypothetical properties of the unknown are derived from what is known, and are then used to fill out the explanation of the known. This kind of circularity is by no means confined to psychology. All scientific hypotheses are shot through with it. The illustration which I chose, and which Köhler attacked, was taken from astronomy.

The first observed positions of Uranus did not permit the calculation of an orbit which could be accounted for by the known motions of adjacent planets. It was therefore assumed that a still more distant planet produced the puzzling deviations in the orbit of Uranus. The hypothetical motions of the unknown planet were derived from the observed positions of Uranus. The behavior of Uranus was explained by the behavior of Uranus. The circularity of the hypothesis was perfect and complete.

Köhler objects to calling this sort of hypothesis circular, for by placing the hypothetical planet within a Newtonian system which is known to operate over a vast range of stellar motions, it is possible to deduce certain consequences which could not possibly be derived from a knowledge of Uranus alone. Some entirely different hypothesis might have been made, in which case it could not be said that both hypotheses were circular with regard to the same known data about Uranus.

"Professor Pratt would answer that in the case of both hypotheses the particular characteristics of the hypothetical agent will have to be computed from the observed behavior of Uranus. This is correct; but on nearer inspection this turns out to be an argument against himself. The hypothesis about an unknown planet leads to certain computations, the explanation by diffuse stellar material to others. Why are the computations different? Simply, because each assumption implies the existence of some factor which exists independently of the irregular behavior of Uranus, and which is a different object in the two cases. Such factors are, however, posited not merely as independent agents; they are also supposed to act according to known laws. In this respect the explanations obviously do not

repeat the content of the observations which are to be explained" (3, p. 122).

There is no serious disagreement here. Köhler admits that in the case of both hypotheses the particular characteristics of the hypothetical agent will have to be computed from the observed behavior of Uranus. The construction of both hypotheses, in other words, will involve circularity. With respect to the manner in which hypotheses are constructed we seem then to be in complete agreement. Köhler goes on to say, however, that my position is untenable because both hypotheses imply the action of some factor which exists independently of Uranus, and that in this respect the explanations obviously do not repeat the content of the observations which are to be explained. The apparent difference of opinion here is due, I think, to the fact that we are not talking about quite the same thing; that Köhler uses the words 'hypothesis' and 'explanation' interchangeably, whereas I should prefer to make a distinction.

It would of course be assumed in the construction of any hypothesis that the agent postulated exists independently of the events seeking explanation. The characteristics of the agent are derived by circular argument, but the agent itself, however hypothetical it may be, is naturally supposed to have an existence somewhere. It is taken to be more than a mere string of words, more than just a phantom of imagination. What would be the purpose of making a guess about a more distant planet to explain Uranus if all the time it was not assumed that there might actually be such a planet?

Let the matter be put in the following fashion. Correlations have been worked out, let us say, between *A*, *B*, and *C*, but they are plainly imperfect and incomplete correlations. The behavior of *A*, *B*, and *C* can only be understood by assuming the existence of another fac-

tor, *d*. The properties of *d* are hypothetical and are derived from the known characteristics of *A*, *B*, and *C*. The hypothetical agent, *d*, is a circular construction, but it is of course assumed that there may be a real *d* somewhere. If *d* is actually discovered, and if it seems to fit in as expected with *A*, *B*, and *C*, let it then be called *D*. The use of *d* as an *assumed* term in a correlation is an *hypothesis*. The ability to include *D* as an *observed* item in a network of correlations constitutes an *explanation*. When Köhler says that in an explanation *D* does not repeat the properties observed in *A*, *B*, and *C*, he is of course right. It is only in the hypothesis that *d* borrows the properties of *A*, *B*, and *C*.

Verbal circularity is presumably characteristic of all hypotheses. The circularity is broken as soon as the agent postulated in the hypothesis proves to be a real event. The initial observations are then included or subsumed under a larger system or network of correlations. Yet it should be realized that although the initial circularity is broken in this manner, the larger tautology of science in general still remains. The discovery of Neptune and the inclusion of both Uranus and Neptune under some law of planetary motion consists in placing a special set of observed correlations within a larger network of known relations. The laws of motion do not *explain* planetary orbits, nor do the motions of planets *obey* these laws, for the laws of motion are themselves summaries of observed correlations. An explanation is the statement of a correlation.

"What is called explaining one law of nature by another, is but substituting one mystery for another; and does nothing to render the general course of nature other than mysterious; we can no more assign a *why* for the more extensive laws than for the partial ones.

... There is often a confused notion that the general laws are the *causes* of the partial ones; that the law of general gravitation, for example, causes the phenomenon of the fall of bodies to the earth. But to assert this would be a misuse of the word cause: terrestrial gravity is not an effect of general gravitation, but a *case* of it; that is, one kind of the particular instances in which that general law obtains" (6, pp. 276 f.).

These words of J. S. Mill represent the view of positivists, operationists, and, I should suppose, the great majority of scientists. It is not a discouraging thesis except to those who would demand of science some penetration into a world not compounded of palpable stuff. There may be such a world, and every human being, including operationists, along with saints, sages, artists, and metaphysicians, presumably entertains some notion as to what it may be like. But it is not a world with which science can deal, although from time to time it is apparently necessary to have a restatement of positivism, such as Bridgman has now furnished, in order to keep science within its proper sphere of activity. Every defense of positivism, however, contains an element of danger, and I suspect it is this element which disturbs Köhler, and perhaps a number of others who seem to dislike positivism. And I must confess that I too am disturbed, partly because of the impression I must have given that a logical circle is vicious. It was my intention to give just the opposite impression, but apparently the phrase 'vicious circle' has established too strong an association in our habits of thought.

In another context Mill says that "an hypothesis being a mere supposition, there are no other limits to hypotheses than those of the human imagination; we may, if we please, imagine, by way

of accounting for an effect, some cause of a kind utterly unknown, and acting according to a law altogether fictitious" (6, p. 290). Mill hastens to add that there are probably few hypotheses in the history of science in which both the agent itself and the law of its operation were entirely fictitious, but he is clearly reluctant to place scientific imagination in a straitjacket. The element of danger in operationism, especially as applied to psychology, lies in just that, *viz.*, the forced restriction of imagination in the selection of problems and the construction of hypotheses.

Question 1 (a). If the laws of physics represent the ideal towards which all other sciences strive, and under which they hope eventually to subsume their special instances of observed correlations, it must follow that psychology, as the latest discipline to make use of scientific method, is still furthest removed from that goal. Does this mean that psychology should abandon the construction of hypotheses until they can be formulated in terms of physics? Certainly not.

The chief value of any hypothesis is heuristic, the stimulation to further research. The construction of an hypothesis ordinarily reveals two stages. In the first stage the observations, operations, and correlations of an experiment are summarized by some phrase, and this phrase, in spite of its circularity, is immediately thought of as a first step towards an explanation. Certain kinds of human and animal behavior are the result of *insight*, intelligence is better understood in terms of *g* or some group of factors, forgetting is traced back to *repression*, etc. These words or phrases can all be given operational definition in terms of what was observed and done in an experiment, or in clinical practice. But the same phrases may also be rephrased in

words borrowed from the laws of physics, the principles of chemistry, from brain physiology or mythology, from common sense or philosophy, even from mysticism. In most cases the rephrasing intends hypothetical agents which are thought of as having a real existence. This second stage is likely to be guesswork and imagination, and is therefore objectionable to some operationists and pure correlationists. There can be no reasonable objection, however, so long as the procedure is regarded as having no more than heuristic value. Some psychologists are stimulated more by brain mythology than they are by cold correlations. Others are stimulated by the fictions of Freud. Whatever stimulates the scientific imagination should be fostered, not discouraged; for there is little danger that the imagination will go too far astray. The operationists will see to that.

The further scientific imagination goes beyond the safe circularity in the first stage of hypothesis-construction, the more improbable it becomes that the agent postulated in the hypothesis can be given operational definition. If the Freudian mechanisms, for example, are thought of as picturesque words which do no more than summarize certain clinical observations, their meaning and definition fall well within the range of operational procedure. But if they are thought of as real independent agents which cause the behavior observed in the clinic, then indeed it would put a strain on operational procedure to specify the nature and location of such agents. In our present state of knowledge any such specification would be impossible. Yet to the psychiatrist and clinician the notion of *repression* as a real force that does things to people is apparently far more useful than to look upon it as a mere word that does no more than summarize what it is supposed to explain. From a practical and

heuristic point of view, the use of *repression* in such a manner would seem to be amply justified. If the justification comes more from a kind of practical sanity possessed by men of science than it does from the pronouncements of operationism, so much the worse for operationism.

It would be unfortunate indeed if the effect of operationism in psychology were to place a stamp of approval on certain limited fields of research in which hypotheses can be neatly formulated in the language of the older sciences, and to look askance at the wide open spaces in which concepts are fluid and vague and sometimes nonsense. The contributions which these limited fields have made to the winning of the war give forceful demonstration to the superiority of facts over theories when effective applications are called for. The search for new and significant facts, however, is made far more effective if directed by hypotheses and theories than if left to unguided exploration. Moreover, we may be entitled to hope that the military demands on the more exact fields of psychological knowledge will before long be a matter of history. And we may also hope that this history will never repeat itself. If psychology is going to help in the realization of this latter hope, its most important contributions will beyond much doubt come from just those wide open spaces in which the pioneers tend to be looked upon by operationists as a bit crude and naive, a bit careless about the niceties and strict usage of exact science.

It may come about after the war that workers in the exact or physical sciences will not look upon psychology with any less respect if it shifts its center of gravity towards those problems, which, however vague and elusive they may be, are vitally important in the understanding of human relations. One of the world's leading astronomers re-

cently referred to group tensions as a problem greatly in need of scientific study (9, p. 31). If the best trained psychologists postpone investigation of that sort of problem on the grounds that exact methods and respectable hypotheses are lacking, the untrained will surely rush in where the best trained fear to tread.

If it is true that positivism in certain of its applications tends to stifle scientific imagination, when is it wise or unwise to make use of operational definitions? The answer to this question, as far as psychology is concerned, is not a matter of either-or, but rather one of degree of emphasis. Most experimental work is preceded by a period of picking and choosing among various problems, a period of vague speculations, of casting up of half-formed hypotheses, of trial-and-error thinking, etc. Little help can or should be expected from operationism in this period.

The value of operationism can make itself felt most effectively at three points after the experimental work is finished: specification of experimental conditions, statement of correlations found, and definition of the concepts used to describe the correlations. The second of these tasks is too obvious to need further comment. The first consists in the construction of a pointer by which the initial data of observation may be identified, a matter which for psychology is still of critical importance in view of the lingering doubts regarding the ontological status of its subject-matter. By far the most crucial task is the third, the definition of concepts used to describe the experimental findings. If the first meaning of these concepts is made identical with the results obtained in

the experiment, then there can be no debate, no confusion, no argument, for the concepts will then intend a set of facts, nothing more, nothing less—and there can be no dispute about facts. Whatever meanings may accrue to the concepts after they have entered into the construction of hypotheses, gone through the minds of different readers, and been transformed and misunderstood and variously handled in their subsequent history, at least the initial meaning can be defined unequivocally and can always be referred to in order to settle controversies. If operationism achieves these ends, it could hardly want to accomplish more, and those who look upon operationism with some misgivings would surely not want it to accomplish less.

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THE OPERATIONAL ANALYSIS OF PSYCHOLOGICAL TERMS

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An answer to Question 6 will define the position to be taken in what follows. Operationism is not regarded as a new theory or mode of definition. The literature has emphasized certain critical or hitherto neglected instances, but no new kind of operation has been discovered and none should be singled out. There is no reason to restrict operational analysis to high-order constructs; the principle applies to all definitions (Question 9). This means, in answer to Question 1 (a), that we must explicate an operational definition for every term unless we are willing to adopt the vague usage of the vernacular.

Operationism may be defined as the practice of talking about (1) one's observations, (2) the manipulative and calculational procedures involved in making them, (3) the logical and mathematical steps which intervene between earlier and later statements, and (4) *nothing else*. So far, the major contribution has come from the fourth provision and, like it, is negative. We have learned how to avoid troublesome references by showing that they are artifacts, which may be variously traced to history, philosophy, linguistics, and so on. No very important positive advances have been made in connection with the first three provisions because operationism has no good answer to Question 10. It has not developed a satisfactory formulation of the effective verbal behavior of the scientist.

The operationist, like most contemporary writers in the field of linguistic and semantic analysis, is on the fence between logical 'correspondence' theories of reference and empirical formulations

of language in use. He has not improved upon the mixture of logical and popular terms usually encountered in casual or even supposedly technical discussions of scientific method or the theory of knowledge (e.g., Bertrand Russell's recent *An inquiry into meaning and truth*). 'Definition' is a key term but is not rigorously defined. Bridgman's original contention that the 'concept is synonymous with the corresponding set of operations' cannot be taken literally, and no similarly explicit but satisfactory statement of the relation is available. Instead, a few round-about expressions recur with rather tiresome regularity whenever this relation is mentioned. We are told that a concept is to be defined '*in terms of*' certain operations, that propositions are to be '*based upon*' operations, that a term denotes something only when there are '*concrete criteria for its applicability*,' that operationism consists in '*referring any concept for its definition to . . . concrete operations . . .*,' and so on. We may accept expressions of this sort as outlining a program, but they do not provide a general scheme of definition, much less an explicit statement of the relation between concept and operation.

The weakness of current theories of language may be traced to the fact that an objective conception of human behavior is still incomplete. The doctrine that words are used to express or convey meanings merely substitutes 'meaning' for 'idea' (in the hope that meanings can then somehow be got outside the skin) and is incompatible with modern psychological conceptions of the organism. Attempts to derive a symbolic

function from the principle of conditioning (or association) have been characterized by a very superficial analysis. It is simply not true that an organism reacts to a sign 'as it would to the object which the sign supplants' (Stevens, 2, p. 250). Only in a very limited area (mainly in the case of autonomic responses) is it possible to regard the sign as a simple substitute stimulus in the Pavlovian sense. Modern logic, as a formalization of 'real' languages, retains and extends this dualistic theory of meaning and can scarcely be appealed to by the psychologist who recognizes his own responsibility in giving an account of verbal behavior.

It is not my intention to attempt a more adequate formulation here. The fundamental revision is too sweeping to be made hastily. I should like, however, to try to make a small but positive contribution to this symposium by considering a few points which arise in connection with the operational definition of psychological terms. Much of the material which follows is adapted from a much longer work now in preparation, in which the necessary groundwork is more carefully prepared.

The operational attitude, in spite of its shortcomings, is a good thing in any science but especially in psychology because of the presence there of a vast vocabulary of ancient and non-scientific origin. It is not surprising that the broad empirical movement in the philosophy of science, which Stevens has shown (2) to be the background of operationism, should have had a vigorous and early representation in the field of psychology—namely, behaviorism. In spite of the differences which Stevens pretends to find, behaviorism has been (at least to most behaviorists) nothing more than a thoroughgoing operational analysis of traditional mentalistic concepts. We may disagree with some of the answers (such as Watson's disposi-

tion of images), but the *questions* asked by behaviorism were strictly operational in spirit. I also cannot agree with Stevens that American behaviorism was 'primitive.' The early papers on the problem of consciousness by Watson, Weiss, Tolman, Hunter, Lashley, and many others, were not only highly sophisticated examples of operational inquiry, they showed a willingness to deal with a wider range of phenomena than do current streamlined treatments, particularly those offered by logicians (e.g., Carnap) interested in a unified scientific vocabulary.) But behaviorism, too, stopped short of a decisive positive contribution—and for the same reason: it never finished an acceptable formulation of the 'verbal report.' The conception of behavior which it developed could not convincingly embrace the 'use of subjective terms.'

A considerable advantage is gained from dealing with terms, concepts, constructs, and so on, quite frankly in the form in which they are observed—namely, as verbal responses. There is then no danger of including in the concept that aspect or part of nature which it singles out. (Several of the present questions seem to mix concept and referent; at least they seem to become trivial when, in order to make the mixture less likely, 'term' is substituted for 'concept' or 'construct.') Meanings, contents, and references are to be found among the determiners, not among the properties, of response. The question 'What is length?' would appear to be satisfactorily answered by listing the circumstances under which the response 'length' is emitted (or, better, by giving some general description of such circumstances). If two quite separate sets of circumstances are revealed, then there are two responses having the form 'length' (Question 2), since a verbal response-class is not defined by phonetic form alone but by its functional rela-

tions. This is true even though the two sets are found to be intimately connected. The two responses are not controlled by the same stimuli, no matter how clearly it is shown that the different stimuli arise from the same 'thing.'

What we want to know in the case of many traditional psychological terms is, first, the specific stimulating conditions under which they are emitted (this corresponds to 'finding the referents') and, second (and this is a much more important systematic question), why each response is controlled by its corresponding condition. The latter is not necessarily a genetic question. The individual acquires language from society, but the reinforcing action of the verbal community continues to play an important rôle in maintaining the specific relations between responses and stimuli which are essential to the proper functioning of verbal behavior. How language is acquired is, therefore, only part of a much broader problem.

We may generalize the conditions responsible for the standard 'semantic' relation between a verbal response and a particular stimulus without going into reinforcement theory in detail. There are three important terms: a stimulus, a response, and a reinforcement supplied by the verbal community. (All of these need more careful definitions than are implied by current usage, but the following argument may be made without digressing for that purpose.) The significant interrelations between these terms may be expressed by saying that the community reinforces the response only when it is emitted in the presence of the stimulus. The reinforcement of the response 'red,' for example, is contingent upon the presence of a red object. (The contingency need not be invariable.) A red object then becomes a discriminative stimulus, an 'occasion,' for the successful emission of the response 'red' (1).

This scheme presupposes that the stimulus act upon both the speaker and the reinforcing community; otherwise the proper contingency cannot be maintained by the community. But this provision is lacking in the case of many 'subjective' terms, which appear to be responses to *private* stimuli. The problem of subjective terms does not coincide exactly with that of private stimuli, but there is a close connection. We must know the characteristics of verbal responses to private stimuli in order to approach the operational analysis of the subjective term.

The response 'My tooth aches' is partly under the control of a state of affairs to which the speaker alone is able to react, since no one else can establish the required connection with the tooth in question. There is nothing mysterious or metaphysical about this; the simple fact is that each speaker possesses a small but important private world of stimuli. So far as we know, his reactions to these are quite like his reactions to external events. Nevertheless the privacy gives rise to two problems. The first difficulty is that we cannot, as in the case of public stimuli, account for the verbal response by pointing to a controlling stimulus. Our practice is to *infer* the private event, but this is opposed to the direction of inquiry in a science of behavior in which we are to predict response through, among other things, an independent knowledge of the stimulus. It is often supposed that a solution is to be found in improved physiological techniques. Whenever it becomes possible to say what conditions within the organism control the response 'I am depressed,' for example, and to produce these conditions at will, a degree of control and prediction characteristic of responses to external stimuli will be made possible. Meanwhile, we must be content with reasonable evidence for the belief that

responses to public and private stimuli are equally lawful and alike in kind.

But the problem of privacy cannot be wholly solved by instrumental invasion. No matter how clearly these internal events may be exposed in the laboratory, the fact remains that in the normal verbal episode they are quite private. We have not solved the second problem of how the community achieves the necessary contingency of reinforcement. How is the response 'toothache' appropriately reinforced if the reinforcing agent has no contact with the tooth? There is, of course, no question of whether responses to private stimuli are possible. They occur commonly enough and must be accounted for. But why do they occur, what is their relation to controlling stimuli, and what, if any, are their distinguishing characteristics?

There are at least four ways in which a verbal community which has no access to a private stimulus may generate verbal behavior in response to it:

(1) It is not strictly true that the stimuli which control the response must be available to the community. Any reasonably regular accompaniment will suffice. Consider, for example, a blind man who learns the names of a trayful of objects from a teacher who identifies the objects by sight. The reinforcements are supplied or withheld according to the contingency between the blind man's responses and the teacher's visual stimuli, but the responses are controlled wholly by tactual stimuli. A satisfactory verbal system results from the fact that the visual and tactual stimuli remain closely connected.

Similarly, in the case of private stimuli, one may teach a child to say 'That hurts' in agreement with the usage of the community by making the reinforcement contingent upon public accompaniments of painful stimuli (a smart blow, tissue damage, and so on). The connection between public and

private stimuli need not be invariable; a response may be conditioned with merely periodic reinforcement and even in spite of an occasional conflicting contingency (1). The possibility of such behavior is limited by the degree of association of public and private stimuli which will supply a net reinforcement sufficient to establish and maintain a response.

(2) A commoner basis for the verbal reinforcement of a response to a private stimulus is provided by collateral responses to the same stimulus. Although a dentist may occasionally be able to identify the stimulus for a toothache from certain public accompaniments as in (1), the response 'toothache' is generally transmitted on the basis of responses which are elicited by the same stimulus but which do not need to be set up by an environmental contingency. The community infers the private stimulus, not from accompanying public stimuli, but from collateral, generally unconditioned and at least non-verbal, responses (hand to jaw, facial expressions, groans, and so on). The inference is not always correct, and the accuracy of the reference is again limited by the degree of association.

(3) Some very important responses to private stimuli are descriptive of the speaker's own behavior. When this is overt, the community bases its instructional reinforcement upon the conspicuous manifestations, but the speaker presumably acquires the response in connection with a wealth of additional proprioceptive stimuli. The latter may assume practically complete control, as in describing one's own behavior in the dark. This is very close to the example of the blind man; the speaker and the community react to different, though closely associated, stimuli.

Suppose, now, that a given response recedes to the level of covert or merely incipient behavior. How shall we ex-

plain the vocabulary which deals with this private world? (The instrumental detection of covert behavior is again not an answer, for we are interested in how responses to private stimuli are normally, and non-instrumentally, set up.) There are two important possibilities. The surviving covert response may be regarded as an accompaniment of the overt (perhaps part of it), in which case the response to the private stimulus is imparted on the basis of the public stimulus supplied by the overt response, as in (1). On the other hand, the covert response may be *similar to*, though probably less intense than, the overt and hence supply the *same* stimulus, albeit in a weakened form. We have, then, a third possibility: a response may be emitted in the presence of a private stimulus, which has no public accompaniments, provided it is occasionally reinforced in the presence of the same stimulus occurring with public manifestations.

Terms falling within this class are apparently descriptive only of behavior, rather than of other internal states or events, since the possibility that the same stimulus may be both public and private (or, better, may have or lack public accompaniments) seems to arise from the unique fact that behavior may be both covert and overt.

(4) The principle of transfer or stimulus induction supplies a fourth explanation of how a response to private stimuli may be maintained by public reinforcement. A response which is acquired and maintained in connection with public stimuli may be emitted, through induction, in response to private events. The transfer is not due to identical stimuli, as in (3), but to coinciding properties. Thus, we describe internal states as 'agitated,' 'depressed,' 'ebullient,' and so on, in a long list. Responses in this class are all metaphors (including special figures like meto-

nomy). The term 'metaphor' is not used pejoratively but merely to indicate that the differential reinforcement cannot be accorded actual responses to the private case. As the etymology suggests, the response is 'carried over' from the public instance.

In summary, a verbal response to a private stimulus may be maintained in strength through appropriate reinforcement based upon public accompaniments or consequences, as in (1) and (2), or through appropriate reinforcement accorded the response when it is made to public stimuli, the private case occurring by induction when the stimuli are only partly similar. If these are the only possibilities (and the list is here offered as exhaustive), then we may understand why terms referring to private events have never formed a stable and acceptable vocabulary of reasonably uniform usage. This historical fact is puzzling to adherents of the 'correspondence school' of meaning. Why is it not possible to assign names to the diverse elements of private experience and then to proceed with consistent and effective discourse? The answer lies in the process by which 'terms are assigned to private events,' a process which we have just analyzed in a rough way in terms of the reinforcement of verbal responses.

None of the conditions that we have examined permits the sharpening of reference which is achieved, in the case of public stimuli, by a precise contingency of reinforcement. In (1) and (2) the association of public and private events may be faulty; the stimuli embraced by (3) are of limited scope; and the metaphorical nature of those in (4) implies a lack of precision. It is, therefore, impossible to establish a rigorous scientific vocabulary for public use, nor can the speaker clearly know himself in the sense in which knowing is identified with behaving discriminately.

tively. In the absence of the 'crisis' provided by differential reinforcement (much of which is necessarily verbal), private stimuli cannot be analysed. (This has little or nothing to do with the availability or capacity of receptors.)

The contingencies we have reviewed also fail to provide an adequate check against fictional distortion of the relation of reference (e.g., as in rationalizing). Statements about private events may be under control of the drives associated with their consequences rather than antecedent stimuli. The community is skeptical of statements of this sort, and any attempt by the speaker to talk to himself about his private world (as in psychological system making) is fraught with self-deception.

Much of the ambiguity of psychological terms arises from the possibility of alternative or multiple modes of reinforcement. Consider, for example, the response 'I am hungry.' The community may reinforce this on the basis of the history of ingestion, as in (1), or collateral behavior associated with hunger, as in (2), or as a description of behavior with respect to food, or stimuli previously correlated with food, as in (3). In addition the speaker has (in some instances) the powerful stimulation of hunger pangs, which is private, since the community has no suitable connection with the speaker's stomach. 'I am hungry' may therefore be variously translated as 'I have not eaten for a long time' (1), or 'That food makes my mouth water' (2), or 'I am ravenous' (3) (compare the expression 'I was hungrier than I thought' which describes the ingestion of an unexpectedly large amount of food), or 'I have hunger pangs.' While all of these may be regarded as synonymous with 'I am hungry,' they are not synonymous with each other. It is easy for conflicting psychological systematists to

cite supporting instances or to train speakers to emit the response 'I am hungry' in conformity with a system. With the balloon technique one might condition the verbal response exclusively to stimulation from stomach contractions. This would be an example of either (1) or (2) above. Or a speaker might be trained to make nice observations of the strength of his ingestive behavior, which might recede to the covert level as in (3). The response 'I am hungry' would then describe a tendency to eat, with little or no reference to stomach contractions. Everyday usage reflects a mixed reinforcement. A similar analysis could be made of all terms descriptive of motivation, emotion, and action in general, including (of special interest here) the acts of seeing, hearing, and so on.

When public manifestations survive, the extent to which the private stimulus takes over is never certain. In the case of a toothache, the private event is no doubt dominant, but this is due to its relative intensity, not to any condition of differential reinforcement. In a description of one's own behavior, the private component may be much less important. A very strict external contingency may emphasize the public component, especially if the association with private events is faulty. In a rigorous scientific vocabulary private effects are practically eliminated. The converse does not hold. There is apparently no way of basing a response entirely upon the private part of a complex of stimuli. *A differential reinforcement cannot be made contingent upon the property of privacy.* This fact is of extraordinary importance in evaluating traditional psychological terms.

The response 'red' is imparted and maintained (either casually or professionally) by reinforcements which are contingent upon a certain property of stimuli. Both speaker and community

(or psychologist) have access to the stimulus, and the contingency may be made quite precise. There is nothing about the resulting response that should puzzle anyone. The greater part of psychophysics rests upon this solid footing. The older psychological view, however, was that the speaker was reporting, not a property of the stimulus, but a certain kind of private event, the sensation of red. This was regarded as a later stage in a series beginning with the red stimulus. The experimenter was supposed to manipulate the private event by manipulating the stimulus. This seems like a gratuitous distinction, but in the case of some subjects a similar later stage could apparently be generated in other ways (by arousing an 'image'), and hence the autonomy of a private event capable of evoking the response 'red' in the absence of a controllable red stimulus seemed to be proved. An adequate proof, of course, requires the elimination of other possibilities (*e.g.*, that the response is generated by the procedures which are intended to generate the image).

Verbal behavior which is 'descriptive of images' must be accounted for in any adequate science of behavior. The difficulties are the same for both behaviorist and subjectivist. If the private events are free, a scientific description is impossible in either case. If laws can be discovered, then a lawful description of the verbal behavior can be achieved, with or without references to images. So much for 'finding the referents;' the remaining problem of how such responses are maintained in relation to their referents is also soluble. The description of an image appears to be an example of a response to a private stimulus of class (1) above. That is to say, relevant terms are established when the private event accompanies a controllable external stimulus, but responses occur at other times, perhaps in

relation to the same private event. The deficiencies of such a vocabulary have been pointed out.

We can account for the response 'red' (at least as well as for the experience of red) by appeal to joint conditions of reinforcement. But what about expanded expressions like '*I see red*' or '*I am conscious of red*'? Here 'red' may be a response to either a public or a private stimulus without prejudice to the rest of the expression, but 'see' and 'conscious' seem to refer to events which are by nature or by definition private. This violates the principle that a reinforcement cannot be made contingent upon the privacy of a stimulus. A reference cannot be narrowed down to a specifically private event by any known method of differential reinforcement.

The original behavioristic hypothesis was, of course, that terms of this sort were descriptions of one's own (generally covert) behavior. The hypothesis explains the establishment and maintenance of the terms by supplying natural public counterparts in similar overt behavior. The terms are in general of class (3). One consequence of the hypothesis is that each term may be given a behavioral definition. We must, however, modify the argument slightly. To say '*I see red*' is to react, not to red (this is a trivial meaning of 'see'), but to one's reaction to red. 'See' is a term acquired with respect to one's own behavior in the case of overt responses available to the community. But according to the present analysis it may be evoked at other times by *any private accompaniment* of overt seeing. Here is a point at which a non-behavioral private seeing may be slipped in. Although the commonest private accompaniment would appear to be the stimulation which survives in a similar covert act, as in (3), it might be some sort of state or condition which gains control of the response as in (1) or (2).

The superiority of the behavioral hypothesis is not merely methodological. That aspect of seeing which can be defined behaviorally is basic to the term as established by the verbal community and hence most effective in public discourse. A comparison of cases (1) and (3) will also show that terms which recede to the private level as overt behavior becomes covert have an optimal accuracy of reference, as responses to private stimuli go.

The additional hypothesis follows quite naturally that being conscious, as a form of reacting to one's own behavior, is a social product. Verbal behavior may be distinguished, and conveniently defined, by the fact that the contingencies of reinforcement are provided by other organisms rather than by a mechanical action upon the environment. The hypothesis is equivalent to saying that it is only because the behavior of the individual is important to society that society in turn makes it important to the individual. The individual becomes aware of what he is doing only after society has reinforced verbal responses with respect to his behavior as the source of discriminative stimuli. The behavior to be described (the behavior of which one is to be aware) may later recede to the covert level, and (to add a crowning difficulty) so may the verbal response. It is an ironic twist, considering the history of the behavioristic revolution, that as we develop a more effective vocabulary for the analysis of behavior we also enlarge the possibilities of awareness, so defined. The psychology of the other one is, after all, a direct approach to 'knowing thyself.'

The main purpose of this discussion has been to answer Question 10 by example. To be consistent the psychologist must deal with his own verbal practices by developing an empirical science of verbal behavior. He cannot, unfortunately, join the logician in defining a definition, for example, as a 'rule for the use of a term' (Feigl); he must turn instead to the contingencies of reinforcement which account for the functional relation between a term, as a verbal response, and a given stimulus. This is the 'operational basis' for his use of terms; and it is not logic but science.

The philosopher will call this circular. He will argue that we must adopt the rules of logic in order to make and interpret the experiments required in an empirical science of verbal behavior. But talking about talking is no more circular than thinking about thinking or knowing about knowing. Whether or not we are lifting ourselves by our own bootstraps, the simple fact is that we *can* make progress in a scientific analysis of verbal behavior. Eventually we shall be able to include, and perhaps to understand, our own verbal behavior as scientists. If it turns out that our final view of verbal behavior invalidates our scientific structure from the point of view of logic and truth-value, then so much the worse for logic, which will also have been embraced by our analysis.

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REJOINDERS AND SECOND THOUGHTS

I

E. G. Boring: Let me first comment on the other five initial contributions to this symposium.

Feigl seems to me to be the expert as well as the clear simple expositor. With him I can go along with no serious dissent. Several times my notes on his paragraphs remark: "Better than I could have said it." Twice he seems to contradict one of my statements (regress of operations vs. ultimate ostensive definitions; limitation of scientific definitions to empirical ones), but the ground for the difference is either practical (concerned with what actually happens) or semantic (turning on what the term *science* should include).

Bridgman I am also able to follow with approval except in part of what he has to remark about discrimination. On certain other matters he has said exactly what I hoped he would say in answer to some of these questions. I believe, however, that a few of my colleagues will think that he has in his present thought modified his more extreme position about the pluralism of constructs which he held in *The Logic of Modern Physics*. After all that book is eighteen years old and Bridgman might be expected to develop his thinking in a couple of decades.

Pratt! I do not know what to say about him. He directs his incisive invective against values that I believe I am prepared to defend, and then he ends up with conclusions that seem perfect to me and beautifully formulated. For instance, he does not want operational definitions of *experience* because they are impossible, whereas I have been arguing that psychology needs operational definitions of *experience* more than of anything else, because *experience* is such a vague and perpetually

misunderstood term in psychological nomenclature. But then it seems to turn out that Pratt and I are in agreement, for it is only *immutably private experience* for which he refuses an operational definition, and even I am not asking for means to publicize the immutably private. We must perforce ignore the immutably private, and Pratt is with me in consigning its definition to metaphysics, and enjoining science to ignore experience which is not publishable.

Skinner is full of his unpublished book and that makes difficulty. He can not get away from the complexities in which his thought is now at home, nor present them fully in the space at his disposal. Again and again I want the referents for his terms. Can many of us be sure what is meant by the sentence: "A verbal community which has no access to a private stimulus may generate verbal behavior in response to it"? In general, I think I follow Skinner, who has limited himself to a discussion of how operationally privacy may be invaded, and agree with him. But he scares me. He has probably implied something that I missed.

Israel would seem to be the only one of the six of us who is, if one dare put it so clumsily, against operationism. He was, of course, the *casus belli* of this symposium. The rest of us may not like the word *operationism* or want to be called *operationists*, but we all agree that operational analysis and definition are scientifically important and that psychology needs them. I shall, therefore, return presently to Israel on equivalence of operations and on purposive behavior.

1. *Empirical foundations* Feigl makes the valid point that there are practical reasons for restricting to science only

the empirical operations, even while admitting that logic and mathematics may also have their defining operations. On the other hand, Skinner is certainly correct in noting that to the basic observations must be added the manipulative and calculational procedures and the logical and mathematical steps that intervene between statements (and, as he says, "nothing else").

2. *Ostensive definitions.* To what I have said about the regression of operations until social agreement is reached should be added Feigl's point that the regress tends to converge upon simpler as well as more generally understood terms, and that the ostensive definition thus in practice provides a limit to the regress, just as it also provides the beginning of meaning in the genesis of the acquisition of language.

3. *Equivalence of operations and the singularity of constructs.* Israel objects to operationism because it breeds a plurality of definitions which, being independent, prevent scientific generalization into constructs. He quotes the Bridgman of 1927: "The concept of length involves as much as and nothing more than the set of operations by which length is determined." Israel believes in the need for general constructs and thinks that operationism is against them. He finds operationism inconsistent in the way it slips from particular operations to absolute constructs. Pratt is dealing with this same point when he discusses the circularity of operational definition and holds that it does not sterilize the construct. Bridgman himself makes, I think, the correct rejoinder in his reply to Question 2 and even better in his reply to Question 11. He even says: "Operational definitions . . . are in practice without significance unless the situations to which they are applied are sufficiently developed so that at least two methods are known of getting to the terminus." Feigl also

seems to me properly to explicate this matter in his reply to Questions 8, 2 and 11. The essence of science lies in generalization and you can not go far in generalization without the use of the generalized abstractions which we call *constructs*. If the operationists had thought that operational analysis would prevent generalization, they would soon have abandoned the sterile pluralistic chaos that would have resulted.

4. *Circularity.* I have the impression that Pratt, in his tilt with Köhler on circularity, does insufficient justice to the equivalence of operations because he does not fully appreciate the symmetry of his astronomical example. Neptune is the observed-and-calculated perturbations of Uranus. Neptune also is the observed speck of light in the telescope. Pratt thinks that the first Neptune was an hypothesis until it was explained by the second Neptune, the speck of light. It seems to me that such logic is reversible, that Neptune is also an hypothesis for the speck of light, one which is explained (reenforced) by Uranus' perturbations. A planet is a whole lot more than a speck of light. You can not really see a construct like a planet in 'direct experience.' You can see a speck of light at a given time and place with a telescope to help you. You can see a perturbation (on the paper on your desk) with enough calculations to help you.

5. *Verbal communication as an operation.* Both Bridgman and Pratt object to the status of ultimacy that Stevens assigns to discrimination when the defining operations for experience are being laid down. Bridgman says that "the discriminatory response is conditioned by the cultural background of the subject," and that such analysis, involving the use of words, is suspect because not all the terms in the operation are known. Just so. To formulate operations in which some of the terms

are words, not the physical data of words but the meanings of words, is a risky business and likely to undo the value of all the operational effort. It is for this reason that Skinner has tried to give us an operational account of how words work and mean in the sample situation of a response to a private stimulus. Skinner is thus anticipating Bridgman's point by indicating how culture actually can be taken into account.

I myself would press Bridgman's point against Allport's dislike of an operational definition of *similarity*. No psychologist can, of course, talk about similarity without making the similarity public (if it was not already public) and the operation of publication is presumably determinable. To object to an operational definition of *similarity* would seem to be merely that you are relying on the identity of meaning for the word *similar* when different people use it. A better operational definition for *similarity* can be formulated, I think, in terms of equivalence or functional substitutability.

It is for this reason that animal experiments are likely to be more clearly formulated than human experiments. Human subjects so often use words, and words need to be defined. The animals also use words, but they are the words of discriminatory response, the words whose history in conditioning performed within the experimental setting fixes their meaning, to the animal and to the experimenter. Bridgman is skeptical of volume, as an attribute of tone separate from pitch and loudness, because the discriminations that are used to get the experimental uniformities depend on the meaning of *volume* and other related spatial words. To some extent I agree with him. If I could get rats consistently to discriminate tonal volume or anything that is the kind of joint function of frequency and intensity that tonal volume is, then I should be much

happier about the status of tonal volume.

No one has ever found out whether any hue-perceiving animal other than man perceives principal colors, but it is invaluable to know how that animal experiment could be performed, as I have indicated in my answer to Question 3(a). To avoid the use of such word-meanings as *pure* and *simple* as applied to colors and to substitute a system of discriminations as defining the meaning of a principal hue is to gain greatly in the precision of the operations employed.

6. *Privacy*. Pratt argues about the privacy of experience. Of course, any atom of existence in the first moment of its being is private to itself before it has come into relation with other atoms. Similarly any event so long as it affects only a single person is private to that person. There is no question about the possible privacy of ultimately scientific data. The point is that science habitually deals with these published privacies, and a privacy that is inherently unpublishable is unknowable (in any rich cognitive sense of *knowable*) and not material for science. It is the immutably private that has no value for psychology or for any science.

This fact means, incidentally, that any datum that has come into psychology must have inherent in it the potentiality of publication and that in itself often adds to our knowledge of the datum, tells us, for instance, something about the relation of brain events to final efferent paths of publication. No one who has learned this lesson is going to be found correlating a datum of 'experience' with an isolated neural event occurring in a particular brain spot at a particular instant.

7. *Purposive behavior*. Israel's second objection to operationism is that it will not, he thinks, handle the definition of *purpose*, a construct which he re-

gards as essential in psychology. He seems to think of purpose as somehow connected with wholes, but of course the operations defining wholes are just as statable as the operations defining elements. But purpose—what is the positivistic account of purpose? Years ago a purpose used to be thought of as a final cause, a way in which the future attracts the present into itself, a sort of temporal suction. Nowadays the goal becomes a drive, the future runs around into the past and pushes the present into the future. Purposes no longer pull; they push, like causes. There is, however, no difficulty about setting up operational definitions of vectors, drives and needs; and, unless one has operational definitions for these dynamic constructs, one is likely to get into trouble with the ambiguity of words. Note what Skinner says about the different meanings of "I am hungry."

It is not impossible that someone may sometime do better with the operational definition of purpose by regarding purpose as the symmetrical opposite of cause, and then noting what purposes correspond in reversed time to the correlations that represent necessary causes and sufficient causes. I throw this out as a suggestion for a positivistic account of purpose, one which does not involve the verbal expression of previously private intuition of intention. The working out of the details turn out, however, to be more involved than one might expect.

In *conclusion* let me say that this symposium has strengthened my faith in operationism immeasurably because of the essential agreements that run through accounts that diverge in detail. I am almost ready to think that operationism is entitled to its *ism* as long as it continues to meet with opposition. An *ism* is a weapon which is always discarded when opposition ceases.

II

P. W. Bridgman: The various discussions have forced again on my attention the curious and almost universal reluctance to accept what seems to me one of the most immediate consequences of the operational point of view. Several of the contributors have referred to science as of necessity being public in character; I believe on the other hand that a simple inspection of what one does in any scientific enterprise will show that the most important part of science is private. I have elaborated this position at some length in a paper¹ and will not repeat here the considerations presented there.

The question with regard to the public or private character of science is only part of the larger question of public versus private in general, which has also been a subject of some discussion in this symposium. An analysis of what I do discloses that in situations in which I am concerned with distinctions between mine and thine my operations are patently dual in character. The operations which justify me in saying, "My tooth aches," are different from those which justify me in saying, "Your tooth aches." The operations which justify me in saying "the toothache which I now have feels like the one which I had last week" are recognizably not the same as those which might justify me in making a similar statement about your tooth aches. Going further, any operations which give whatever meaning they may have to statements like "my toothache feels like your toothache" are obviously compounded of the operations which separately give meaning to my toothache and your toothache. In fact, this situation is so obviously a compound of heterogeneous elements that it is coming to be a rather common point of

¹ Science: public or private? *Phil. Sci.*, 1940, 7, 36-48.

view that the best way to treat such questions as "does your toothache feel like my toothache?" is to call them pseudo questions. In general, the operations by which I know what I am thinking about are different from the operations by which I convince myself of what you are thinking about. The question never arises, "Am I deliberately deceiving myself with regard to what I am thinking about?" but the question often arises as to whether you are deliberately deceiving me with regard to what you are thinking about.

The whole linguistic history of the human race is a history of a deliberate suppression of the patent operational differences between my feelings and your feelings, between my thought and your thought. A language which reproduced the dualistic character of what happens would have different words for your thought and my thought. The reason for the suppression of the distinction and the use of a single word is doubtless social. We understand and manage to get along with our fellows by the device of saying "my neighbor has feelings exactly like mine." It is easy to imagine that the possession of this linguistic device may have been of universally decisive survival value. It by no means follows, however, that a linguistic usage which has arisen under the stimulus of an immediate social necessity is the most advantageous or is even adequate to meet the complete scientific requirements. There is little connection between survival value and truth.

The topic to which Professor Skinner devotes the major part of his discussion is obviously intimately related to what we are considering here, but it is recognizably not the same. Professor Skinner is concerned with how to treat the reactions of my neighbor to stimuli which we would all describe as private to him. I think it must be conceded

that Professor Skinner is right in his contention, if I understand him correctly, that the only possible way of dealing with this problem is to convert "private for my neighbor" into "public for me." I think, however, that there is danger that this scheme of procedure may involve the tacit thesis that it is possible to go further and establish the full operational equivalence of "public for me" and "private for me." This equivalence may conceivably ultimately be established, perhaps by an elaboration of methods suggested by Professor Skinner, but it is at any rate plain that the equivalence has not yet been established. The most superficial observation is sufficient to show that the operations by which I now deal with the "public for me" are qualitatively different from the operations by which I now deal with the "private for me." To ignore this difference, or to set up the thesis that the difference is unimportant until it is proved, is opposed to the entire spirit of the operational approach. It may be objected that it would lead to impossible complication to insist on the differences between public and private—that a strict application of this point of view would mean that there are as many "sciences" as there are people engaged in "sciencing." This may indeed be the case, but if it is we can do nothing about it but accept it. The first consideration must be "what is true?" "what is simple?" I believe that it is always the first result of a careful operational analysis will be to find complication rather than simplification. The conceptual structure which we inherited is a complicated and simplified structure in which we do not know what the simplifications are or what are their consequences. The first task of the operational approach is to recover the complexity of the operative situation. I suspect that most persons will

'practical' frame of mind will have little patience with these considerations, because they believe they already know the answer, and that considerations of this sort can in the end make little or no difference with any of our procedures. This attitude is of course an exceedingly dangerous attitude and has often led to disaster in the past. It is my own considered opinion that the matter is of transcendent importance. The entire human race, ever since the appearance of articulate speech, has been so conditioning itself to suppress the difference between me and thee that most members of the race have lost any capacity they may ever have had to recognize even the existence of the issue. Simple observation shows that I act in two modes. In my public mode I have an image of myself in the community of my neighbors, all similar to myself and all of us equivalent parts of a single all-embracing whole. In the private mode I feel my inviolable isolation from my fellows and may say, "My thoughts are my own, and I will be damned if I let you know what I am thinking about."

All government, whether the crassest totalitarianism or the uncritical and naive form of democracy toward which we are at present tending in this country, endeavors to suppress the private mode as illegitimate, as do also most institutionalized religions and nearly all systems of philosophy or ethics. Yet the private mode is an integral part of each one of us, ready to flare into action under the stimulus of any new exploitation of the individual. I believe that no satisfactory solution will be found for our present social and political difficulties until we find how to handle together as of equal importance the social and the private modes of each of us. Each of us, in moments of clarity or stress, reverts to the private mode in spite of millenia of exhortation

and instruction. In these moments of clarity we know that the private mode is as justifiable as the social mode and even more inescapable. It seems to me that only when I deal with both modes do I become capable of achieving complete rationality. No government or social order can be ultimately successful, if its members are intelligent and allowed to follow their own intellectual processes to their logical conclusions, until a reconciliation has been achieved between these two modes. In fact, it seems to me that this is the supreme justification for the sort of democracy toward which we ought to be heading but unfortunately are not, namely, that it alone makes sense from the point of view of the completely rational behavior of the individuals who compose it.

The extent to which any discipline suffers by its failure to recognize and insist on the social and the private modes of individual behavior depends on the subject matter. In physics the question hardly presents itself. But in psychology it seems to me that we do want to deal with topics which demand a clear recognition of the operational duality with which at present we are constrained to deal with all questions of me and thee. To assume that this operational duality may be ignored assumes the result of what is at present only a program for the future. In the light of present accomplishment this assumption seems to me exceedingly hazardous. Until it has been shown that the program has reasonable prospects of being carried through the operational approach demands that we make our reports and do our thinking in the freshest terms of which we are capable, in which we strip off the sophistications of millenia of culture and report as directly as we can what happens. Among other things this demands that I make my reports always in the first person

and in a language which reproduces the structure of my universe. Since one aspect of the structure of my universe is the operational difference between mine and thine, I cannot make my report in a language which recognizes this operational duality. For such a language does not a positivist, one must be devised. For the present it will probably be sufficient to use such words as *thought* or *feeling* without qualification, but always to qualify, as "my thought," "your thought," or "my feeling," "your feeling." It may be that eventually we shall be able to take account of the operational dichotomy in the universes of each one of us in some simpler way. But until that time, it seems to me that by ignoring the dichotomy psychology is engaging in an unnecessary gamble, and is probably riding for a fall.

III

Herbert Feigl: Owing to limitations of space, the theme of my rebuttal will be restricted to the concept of scientific explanation. Instead of presenting a point by point polemical discussion of some of the views of my fellow-symposiasts I shall try to clear up the issue mainly by a concise statement of my own analysis of the explanatory procedure. The reader may then compare this account with that of Bridgman and especially the one of Pratt.

Some positivistic and operationistic definitions of 'scientific explanation' have all too narrowly stressed: (1) that there is no fundamental difference between *description* and *explanation*; (2) that all scientific explanation is *circular* or *tautological*; (3) that in explanation we *reduce the unfamiliar to the familiar*. I would urge that there is something basically wrong, or in any case, something very misleading, in all three contentions. A modern logical empiricism may retain the valuable anti-

metaphysical tendency in the old point of view while at the same time giving a methodologically more adequate reconstruction of the explanatory process as actually employed in various sciences. It is agreed that scientific explanation differs from the pseudo-explanation animistic, theological or metaphysical types in that the explanatory of legitimate science must be testable, and must not be superfluous (not redundant in the light of the principle of parsimony). The truth of the premises and the truth of pseudo-explanations is emotive, i.e., pictorial and non-testable. It is also agreed that all legitimate explanation is never absolute but relative, in the following two regards: (1) any given explanation proceeds from premises which, although possibly subject to further explanation, are *assumed* or *taken for granted* in the given case; (2) is only at the price, and in the light of such assumptions that we can explain for the explicanda; (3) the explanatory premises, as regards their truth, are relative to the confirming evidence and therefore subject to revision.

My terminological suggestion amounts to the definition of 'explanation' as the inductive or (on higher levels) hypothetico-deductive derivation of the more specific (ultimately descriptive) propositions from more general assumptions (laws, hypotheses, theoretical postulates) in conjunction with other descriptive propositions (and often together with definitions). 'Explanation' is thus taken primarily as *procedure of inference* (just like the closely related 'prediction'), with the only permissible alternative of the substantial use that calls the *premises* in those deductive 'explanations' of the facts to be explained (as formulated in the conclusion). The 'necessity' which is bes-

upon the facts by their explanation is the *logical necessity* of the *implication* underlying the inference from assumptions to conclusions. Neither the premises nor the conclusions in explanatory inferences of the empirical sciences are logically necessary in and by themselves. Only in a purely mathematical proof, such as we find in arithmetic or algebra, premises as well as conclusions may in themselves be logically necessary (analytic).

It is very helpful to restrict the meaning of 'description' to singular statements representing fully specific facts, events or situations. Such descriptions may appear as the conclusions of explanatory, *i.e.*, deductive inferences. Some of the premises of these inferences must then be scientific laws or theoretical assumptions. Since laws and theoretical assumptions are (or at least contain) generalized statements (*i.e.*, unlimited universal propositions) they are not here classified as descriptions. They are the premises of explanatory or predictive (deductive) inferences and thus are themselves essentially of inductive validity. Sometimes these laws or assumptions may be more familiar than the conclusions. The whole trend of mechanistic explanation manifests this tendency toward familiarization. But since, even in physics, this mechanistic trend found its very definite limitations, and since, particularly in the scientific achievements of the last eighty years, the trend has often been reversed, we may say that very frequently the

well known, long familiar facts have been explained by principles only much more recently discovered and lacking tang of familiarity. (Among dozens of examples bearing out this point I will only mention the electromagnetic explanations of the familiar properties of light; the quantum-mechanical explanations of chemical processes; or the neuro-physiological explanations of sensory or reflex phenomena). That which matters is thus not the familiarity but the *generality* of the explanatory premises.

Since generality is a matter of degree, or rather of level, it is useful to distinguish *levels of explanation*. The empirical (*i.e.*, experimental, or else, statistical) laws which function as premises in the deductive derivation of strictly descriptive conclusions may in turn become the conclusions of a super-ordinated deductive derivation from higher theoretical assumptions. In principle this process could repeat indefinitely but in practice it is usually found to stop at a second or third level. There is neither a danger of nor a need for an infinite regress. The top level at any given stage of theoretical research (in the ideal case) simply covers all relevant and available descriptive data; and there is no need for climbing higher on the tower of constructs if all the data one cares to see are within sight.

It seems convenient to represent the levels of explanation along the lines of the following scheme (read from bottom up!):

Theories 2nd order	Still more penetrating interpretation (still higher constructs)
Theories 1st order	Sets of assumptions using higher-order constructs (results of abstraction and inference). (Deeper interpretation of the facts as rendered on the Empirical Law-level)
Empirical Laws	Functional relationships between relatively directly observable (or measurable) magnitudes
Description	Simple account of individual facts or events (data) as more or less immediately observable

In actual scientific practice the distinctions, as well as the number, of levels are neither quite as sharp or fixed as suggested here. The scheme is offered merely as a suggestion toward a first orientation. The question 'why' (in the sense of a demand for explanation) is answered by deduction either from empirical laws or from theories. Deduction from empirical laws may be styled 'low-grade' explanation. It merely puts the fact to be explained into a class of facts characterized by the same empirical law. Thus the explanation for the fact, *e.g.*, that there is a mirror image of a bridge in a river, is achieved by subsuming this fact under the law of reflection in geometrical optics. This law is simply the common denominator of all the various phenomena in which light-reflection is the essential feature. A 'higher-grade' explanation we find in the Maxwell-electromagnetic wave theory, which serves as a basis for deduction for a variety of optical phenomena: reflection as well as refraction, diffraction, interference, dispersion, polarization, etc., etc. It is on this theoretical level (the "row of genius" as I like to call it) that we gain a "real insight into the nature of things" (as metaphysicians call it). What we give on this level are interpretations concerning the *structure* of light, of matter, of electricity, etc. The constructs of this theoretical level usually concern the micro-structure of the observed macro-phenomena, *i.e.*, they involve existential assumptions (atom, electron, photon-hypotheses) or constructs of the abstract mathematical order (energy, entropy, tensors, probability functions, etc.). No wonder that the 'Aha-experience' is much stronger for these deductions from theories than for the much simpler deductions from empirical laws.

Once the theoretical concepts are properly introduced, they can be used also for purposes of description on the

lowest level—*e.g.*, Einstein can describe the physical state of a given volume of space in terms of 14 highly theoretically defined magnitudes. Similarly Tolman or Hull can *describe* the behavior of an organism in terms of the intervening variables of their respective systems.

The question regarding *circularity* may be resolved by defining what is usually called an *ad hoc* explanation. Now, an *ad hoc* explanation is deceptive because it has only the external form of a 'real explanation.' It is *ad hoc* in that it explains *only* the fact which it was to explain (*i.e.*, for the sake of which it was introduced). It may be either purely verbal, *e.g.*, "Birds build nests because they have nest-building instincts." Or it may be unscientific in that it assumes entities which do not manifest themselves in any other way (explanation of Gravitation by Lesage: particle-radiation, etc.) or it may down-right metaphysical, if the explanatory hypotheses are in principle incapable of test (such as the assumptions of entelechies, vital forces in vitalistic and animistic biology and psychology). Of course, everything depends on how the explanatory phrases are interpreted; the use of the word 'instinct' can be quite legitimate (and more than purely verbal) if, *e.g.*, it is meant in the sense of an empirical regularity in the behavior of a species. Then it is a 'low-grade' explanation, possibly preparing the way for a 'higher-grade' theoretical explanation (on the basis of a physiological theory of heredity, maturation, etc.) Similarly explanations of rapidity of learning on the basis of 'intelligence' are not purely verbal (or *ad hoc*) but *low-grade* explanation on the basis of empirical facts. That is, as long as the 'intelligence quotient' refers to various types of capacities, various types of learning activities, it enables us to relate the

to the others *via* the common factor, 'I.Q.'

In some cases the reproach of 'circularity' is made against 'low-grade explanation' *if* it pretends to be 'high-grade.' But it seems there is no absolutely sharp line between the two—because sets of empirical laws sometimes function very much like theoretical assumptions of the higher construct type.

More fundamentally and logically speaking the contention of circularity or tautologicality in scientific explanation is right in one interpretation but definitely wrong in another: It is right if it stresses the analytic (*i.e.*, strictly logical, sometimes called 'tautological') character of the deductive inference leading from premises to conclusion in any explanatory argument. In a more precisely definable sense it can be said that the conclusion is 'contained' in the conjunction of the premises. The charge of circularity or of *petitio principii* is justified only if either the conclusion appears literally as one of the premises or if the truth of one of the premises is proved by appeal to the conclusion. The customary procedure of the hypothetico-deductive method in the empirical sciences is perfectly capable of avoiding both sources of circularity. The (psychological) novelty sometimes amounting to surprise (Heureka!), in the more advanced and worthwhile instances of scientific explanation shows that the conclusion was not one of the premises. And the truth of the explanatory assumptions is always only suggested (*i.e.*, confirmed to some degree) but never fully proved by evidence which is distinct from the facts to be explained. Newton's law of gravitation together with his laws of mechanics were already highly confirmed by the facts of planetary motion, by the orbits of satellites, comets, and many other items of evidence, when Leverrier and Adams used those laws

as explanatory premises, together with the existential hypothesis regarding the orbit of another up to then not observed planet, in order to explain the irregularities of Uranus' motion. True, by a 'tautological' (better: deductive) transformation the conclusion (concerning Uranus' path) was derived from premises (laws of mechanics, law of gravitation, etc.) but the major premise says infinitely more than the conclusion and it is therefore not possible to deduce the premises from the conclusion. Pratt overlooks the inductive leap, the leap from 'this' to 'all' in explanatory generalizations. By declining to differentiate sharply between explanation and description Pratt views generalizations as descriptions. 'Description' thereby loses its ordinarily precise meaning and the distinction between fact on the one hand, and law or theory on the other, is in danger of being blurred or even obliterated.

Moreover, the hypothesis of the existence of a further planet (after its telescopic discovery called 'Neptune') was suggested by the analogy with the facts regarding the then known planets. The inductive probability of this existential hypothesis therefore (at the time of Leverrier and Adams) did not rest exclusively on the *ad hoc* or circular procedure described by Pratt but had an independent foundation, no matter how weak or strong, in the already established body of astronomical knowledge. Similarly in psychology: However vague and uncertain Freud's original hypotheses regarding the repressed or unconscious parts of the mind may have been, they were *not* circular in the sense of *ad hoc*. He was guided by analogies of the conscious and pre-conscious and was able to unify through his hypothesis a great number of previously unrelated facts, such as certain types of forgetting, slips and lapses, dreams, hysterical and neurotic symp-

toms, etc.—A methodologically similar situation prevails also in psycho-physiology.

To summarize: A scientific explanation is free from objectionable circularity or *ad hoc* character if it helps connecting hitherto unconnected specific facts ('low-grade' explanation) or laws ('high-grade' explanation). While the deductive part of the hypothetico-deductive procedure may be said to be 'tautological' or analytic (in the sense in which the classical syllogism is valid only if a denial of the conclusion strictly implies a denial of at least one of the premises); but it is not *circular*, (in the sense that the conclusion be logically equivalent with one of the premises or that the conclusion itself be the sole basis of the inductive probability of the hypothetical premise).

Note: Very briefly replying to the critical questions of Israel, particularly those raised in his joined article with Goldstein (PSYCHOL. REV., 1944, 51, No. 3), I should like to state that I see no such cleavage between the operationism of the physicists and that of psychologists as is there emphasized. It is true that Bridgman's exemplifications usually involve the mensurational aspects of physical constructs. But aside from these aspects there are others: purely qualitative (classificatory), causal-genetic or dispositional aspects of concept-formation, often preceding the quantitative-functional or the theoretical-constructive. Such physical concepts (to choose at random) as: magnetism, radio-activity, photo-electric sensitivity, phosphorescence, etc., before they attain metrical status or theoretical interpretation, are introduced (if you will: 'defined') on the level of empirical laws by conditional definitions (involving reference to test situations). This is very much as it is in psychology with such concepts as personality traits, habits, drives, latent dispositions, etc. The fact that the introduction of these concepts requires reference to experimental or testing situations has been misinterpreted by pragmatists and

some operationists in construing it as a 'production' of the pertinent properties or reactions. But there is no *basic* difference between the purely observational or mensurational procedures of a science, say like astronomy, and the experimental sciences where, according to the pragmatist interpretation, the intervention or interference of the scientist's actions is essential and indispensable. What really matters in either case is that we observe what happens under what conditions,—be these conditions 'naturally given' or 'artificially set up.'

References: The confusions, particularly in J. S. Mill's *Logic*, regarding the circular character of deductive inference were well criticized already by J. N. Keynes in his *Studies and Exercises in Formal Logic* (4th ed.), London, 1904, Section 381, p. 474 ff., and again, very pointedly, by R. M. Eaton in *General Logic*, New York, 1931, p. 140 ff. A very lucid discussion of the concept of scientific explanation with special reference to the social sciences may be found in C. G. Hempel's article 'The function of general laws in history,' *J. Phil.*, 1942, 39, p. 35 ff. For a very elaborate discussion of explanation in physics, see N. R. Campbell, *Physics: The Elements*, Cambridge Univ. Press, 1921. Also G. Bergmann, 'Outline of an empiricist philosophy of physics,' *Amer. J. Physics*, 1943, 11, pp. 248-258; 335-342. Further excellent discussions of operationism and explanation in psychology: in the articles by G. Bergmann and K. W. Spence (PSYCHOL. REV., 1941, 48, pp. 1-14) and by K. W. Spence (PSYCHOL. REV., 1944, 51, pp. 47-68). Nor should the, by now classical, article by K. S. Lashley (PSYCHOL. REV., 1923, 30) be forgotten.

IV

Carroll C. Pratt: Skinner's contention that "behaviorism has been (at least to most behaviorists) nothing more than a thoroughgoing operationa' analysis of traditional mentalistic concepts () must come as a surprise to psychologists who have us tional mentalistic concepts, b most behaviorists. His con this score, whether shared by not, is significant, for it im traditional or classical psych

sound enough in its choice of subject-matter and experimental procedures, but went astray only in the realm of logic and definition. Yet Skinner gives himself a good deal of trouble defending the classical psychologist's use of a subject-matter which the traditionalists would hardly have supposed, before the advent of behaviorism, needed any defense or justification at all. "There is nothing" says Skinner, "about the resulting response ('red') that should puzzle anyone. The greater part of psychophysics rests upon this solid footing" (). He goes on to say, however, that the older psychological view had it "that the speaker was reporting, not a property of the stimulus, but a certain kind of private event, the sensation of red. . . . This seems like a gratuitous distinction" (). If the fault of the older psychology was in assuming that red is not a property of the stimulus, but an event which requires an intact optical system inside the speaker who reports the red, then at least it was in good company. To call such a distinction gratuitous is to maintain some sort of behavioristic realism which is rather more naive than critical.

Skinner's distrust of the traditional treatment of phenomena like red seems linked with his abhorrence and rejection of events that conceivably might be contaminated with privacy in their initial status. In view of the fact that four of the contributors to the present symposium (Boring, Feigl, Skinner, and myself) were disturbed by the question of private *vs.* public data in psychology, my own statement to the effect that it is an anachronism for psychology still to be bothered by the inescapable solipsism that enters into the problem of translating private into public events must be laid to wishful thinking on my part. Yet I can do no more in this brief rebuttal than continue to think with the same wish.

An operationism that operates upon the data of psychology by principles of exclusion rather than inclusion stands condemned at the very outset. It is not the task of operationism to enumerate, evaluate, and then eliminate initial data. Any kind of data are suitable materials for scientific investigation provided their description and report can be made palpable and ostensible to workers interested in pursuing them. Skinner's treatment of the semantic refinements involved in translating the privacy of a toothache into some kind of publicity which the verbal community will acknowledge may be an excellent exercise in behaviorism, but it smacks dangerously of sterile operationism, and does nothing to remove the initial privacy that surrounds every toothache. *Every datum in every science starts life as a bit of private experience.* Nothing written in this symposium, nor for that matter in any of the literature on operationism or positivism that I have read, has been able to rid me of that dogmatic conviction.

Feigl's attempt to circumvent the problem of initial privacy seems hardly more than a play on words. The statement that "private, immediate experience as such is only the raw material, not the real subject-matter of science" () calls for an elaboration, which Feigl does not give, of what he means by "the real subject-matter of science." What indeed is the real subject-matter of science, and how does it differ from the raw material of science? "One's own immediate experience" says Feigl, "is . . . that small foothold in reality that any observer must have in order to get at all started in his business of exploring the world of things and organisms surrounding him" (). With that statement there can hardly be any quarrel, unless one were to quibble about the size of the foothold that immediate experience offers. However restricted the

universe of immediate experience may be, it is certainly of vital importance, for the simple and sufficient reason that there is no other universe to go to to find the initial data for science. Yet Feigl contends that if one restricts himself to that universe "the scientific value of such a study is very insignificant as long as it remains unrelated to a study of extra-dermal and intra-dermal stimuli (physical and physiological processes) or to behavior responses" (). Such a study would indeed be very insignificant, except perhaps in preliminary phases of exploration in a new field. But where in psychology can one find any serious attempt to deal with observations in any such vacuum as would be created by sealing them off from their connections with extra-dermal and intra-dermal stimuli?

Feigl seems to share the occasional behaviorist's notion that classical psychologists were phenomenologists pure and undefiled who dozed in their armchairs and dreamed a literary psychology of precious and useless privacy. Yet I suspect he would be hard put to it to demonstrate in what respect the work of psychophysicists was any less scientific and public than that of the behaviorists. It is therefore not clear what predilections dictate his avowed preference for one kind of psychological datum over any other kind. He says that "if one wishes to convert this narrow realm of directly given experience into subject-matter for description one is limited to a purely phenomenological approach of the introspective type" () and then goes on to say that "there is the alternative of a strictly physicalistic or behavioristic approach right from the start" (). I could wish that Feigl, as philosopher, logician, and positivist, would clarify the difference between phenomenism and physicalism as applied to the initial data of psychology, or of any other science. If that matter

were clarified, it might then be possible to understand why he is so strongly convinced that "from the point of view of the methodology of science this (*i.e.*, the behavioristic approach) is preferable, since it eliminates with one stroke the pitfalls of the traditional metaphysical pseudo-problems of solipsism, the mind-body puzzle, etc." ().

A philosopher who has embraced positivism should be the last person to be misled by traditional and conventional meanings of words. In the present context Feigl seems to imply that physicalism, behaviorism, and objective events belong in one universe, and that phenomenism, introspectionism, and subjective events belong in another; and that the two are not only fundamentally different, but that the first is the only proper universe for science, whereas the second is full of metaphysics, solipsism, and mind-body puzzles. This dichotomy only serves to revive and perpetuate the very mind-body problem which Feigl oddly enough seems to regard as disposed of by preserving a cleavage between the physical and mental, the public and private, the objective and subjective.

Operationism should help to rid psychology of the mind-body problem and its various ramifications, but this purpose can hardly be achieved by stirring up the same old dust raised by the quarrel between behaviorism and introspectionism two decades or more ago. Introspection was supposed to deal with the privacy of subjective events, behaviorism with the publicity of objective events. If some kind of critical positivism or operationism could have intervened in the early stages of the dispute, the unprofitable war of words might have terminated sooner, for most of the trouble arose over what turned out to be the rather naive assumption by both the traditionalists and the behaviorists that introspection meant a

turning of the mind inward to observe the subjective stream of consciousness, —about as meaningless a succession of words as one could hope to string together.

The first item in the formulation of any scientific concept is some datum of direct observation, and the last item in the definition of any scientific concept should again be some datum of observation. "A series of definitional steps may be long," as Feigl himself points out, "but it will terminate with definientia which are linked to something outside the realm of terms and symbols—namely, items of direct observation" (). Ostensive definitions of this kind come to an end in one and only one universe, the one to which all science must go for the initial materials of observation. These materials can only be pointed to. They can not be placed in ontological categories of physical, mental, physiological, chemical, phenomenal, behavioral, *etc.* These labels are acquired in the realm of logic and concept-formation, and are sometimes useful as indications of divisions of labor within science. Ordinarily the use of such words is relatively harmless, but they may at times be utterly confusing, as in the case of behaviorism *vs.* introspectionism, and should under no circumstances ever be applied to the initial data of observation themselves.

Neither physics nor psychology can completely rid itself of the problem created by the initial privacy which surrounds all human observation and report, and both disciplines are equally involved in the metaphysical question of the body-mind relation. It is worthy of note, however, that the distinguished physicist in our midst, whose name is so closely associated with operationism and the critical examination of scientific concepts, seems not at all disturbed by metaphysics, perhaps because he believes that operationism can help to

keep metaphysical problems at a respectful distance from science. The rest of us should learn a lesson from Professor Bridgman.

V

B. F. Skinner: In the summer of 1930, two years after the publication of Bridgman's *Logic of Modern Physics*, I wrote a paper called 'The concept of the reflex in the description of behavior.' It was later offered as the first half of a doctoral thesis and was published in 1931. Although the general method, particularly the historical approach, was derived from Mach's *Science of Mechanics*, my debt to Bridgman was acknowledged in the second paragraph. This was, I think, the first psychological publication to contain a reference to the *Logic of Modern Physics*, and it was the first explicitly operational analysis of a psychological concept.

Shortly after the paper was finished, I found myself contemplating a doctoral examination before a committee of whose sympathies I was none too sure. Not wishing to wait until an unconditional surrender might be necessary, I put out a peace feeler. Unmindful or ignorant of the ethics of the academy, I suggested to a member of the Harvard department that if I could be excused from anything but the most perfunctory examination, the time that I would otherwise spend in preparation would be devoted to an operational analysis of half-a-dozen key terms from subjective psychology. The suggestion was received with such breathless amazement that my peace feeler went no further.

The point I want to make is that at that time—1930—I could regard an operational analysis of subjective terms as a *mere exercise in scientific method*. It was just a bit of hack work, badly needed by traditional psychology, which I was willing to engage in as a public service or in return for the remission of

sins. It never occurred to me that the analysis could take any but a single course or have any relation to my own prejudices. The result seemed as predetermined as that of a mathematical calculation.

In spite of the present symposium, I am of this opinion still. I believe that the data of a science of psychology can be defined or denoted unequivocally, and that some one set of concepts can be shown to be the most expedient according to the usual standards in scientific practice. Nevertheless, these things have not been done in the field which was dominated by subjective psychology, and the question is: Why not?

Psychology, alone among the biological and social sciences, passed through a revolution comparable in many respects with that which was taking place at the same time in physics. This was, of course, behaviorism. The first step, like that in physics, was a reexamination of the observational bases of certain important concepts. But by the time Bridgman's book was published, most of the early behaviorists, as well as those of us just coming along who claimed some systematic continuity, had begun to see that psychology actually did not require the redefinition of subjective concepts. The reinterpretation of an established set of explanatory fictions was not the way to secure the tools then needed for a scientific description of behavior. Historical prestige was beside the point. There was no more reason to make a permanent place for 'consciousness,' 'will,' 'feeling,' and so on, than for 'phlogiston' or '*vis anima*.' On the contrary, redefined concepts proved to be awkward and inappropriate, and Watsonianism was, in fact, practically wrecked in the attempt to make them work.

Thus it came about that while the

behaviorists might have applied Bridgman's principle to representative terms from a mentalistic psychology (and were most competent to do so), they had lost all interest in the matter. They might as well have spent their time in showing what an eighteenth century chemist was talking about when he said that the Metallic Substances consisted of a vitrifiable earth united with phlogiston. There was no doubt that such a statement could be analyzed operationally or translated into modern terms, or that subjective terms could be operationally defined. But such matters were of historical interest only. What was wanted was a fresh set of concepts derived from a direct analysis of the newly emphasized data, and this was enough to absorb all the available energies of the behaviorists. Besides, the motivation of the *enfant terrible* had worn itself out.

I think the Harvard department would be happier today if my offer had been taken up. What happened instead was the operationism of Boring and Stevens. This has been described as an attempt to climb onto the behavioristic band-wagon unobserved. I cannot agree. It was an attempt to acknowledge some of the more powerful claims of behaviorism (which could no longer be denied) but at the same time to preserve the old explanatory fictions unharmed. The strategy adopted is more apparent in Boring's present paper than in Stevens' earlier publications. A concession is made in accepting the claim that the data of psychology must be behavioral rather than mental if psychology is to be a member of the United Sciences, but the position taken is merely that of 'methodological' behaviorism. According to this doctrine the world is divided into public and private events, and psychology, in order to meet the requirements of a science, must confine itself to the

former. This was never good behaviorism, but it was an easy position to expound and defend and was often resorted to by the behaviorists themselves. It is least objectionable to the subjectivist because it permits him to retain 'experience' for purposes of self-enjoyment and 'non-physicalistic' self-knowledge.

The position is not genuinely operational because it shows an unwillingness to abandon fictions. It is like saying that while the physicist must admittedly confine himself to Einsteinian time, it is *still true* that Newtonian absolute time flows 'equally without relation to anything external.' It is a sort of *E pur si muove* in reverse. What is lacking is the bold and exciting behavioristic hypothesis that what one observes and talks about is always the 'real' or 'physical' world (or at least the 'one' world) and that 'experience' is a derived construct to be understood only through an analysis of verbal (not, of course, merely vocal) processes.

The difficulties which arise from the public-private distinction have a prominent place in the present symposium, and it may be worth while to consider four of them.

(1) The relation between the two sets of terms which are required has proved to be confusing. The pair most frequently discussed are 'discrimination' (public) and 'sensation' (private). Is one the same as the other, or reducible to the other, and so on? A satisfactory resolution would seem to be that the terms belong to conceptual systems which are not necessarily related in a point-to-point correspondence. There is no question of equating them or their referents, or reducing one to the other, but only a question of translation—and a single term in one set may require a paragraph in the other.

(2) The public-private distinction

emphasizes the arid philosophy of 'truth by agreement.' The public, in fact, turns out to be simply that which can be agreed upon because it is common to two or more agreeers. This is not an essential part of operationism; on the contrary operationism permits us to dispense with this most unsatisfying solution of the problem of truth. Disagreements can often be cleared up by asking for definitions, and operational definitions are especially helpful, but operationism is not primarily concerned with communication or disputation. It is one of the most hopeful of principles precisely because it is not. As Boring admits, the solitary inhabitant of a desert isle could arrive at operational definitions (provided he had previously been equipped with an adequate verbal repertoire), and I cannot see why these would not be physicalistic. It is a little far-fetched to bring in self-communion in order to preserve the principle of truth by agreement. The ultimate criterion for the goodness of a concept is not whether two people are brought into agreement but whether the scientist who uses the concept can operate successfully upon his material—all by himself if need be. What matters to Robinson Crusoe is not whether he is agreeing with himself but whether he is getting anywhere with his control over nature.

One can see why the subjective psychologist makes so much of agreement. It was once a favorite sport to quiz him about inter-subjective correspondences. 'How do you know that *O*'s sensation of green is the same as *E*'s?' And so on. But agreement alone means very little. Various epochs in the history of philosophy and psychology have seen whole-hearted agreement on the definition of psychological terms. This makes for contentment but not for progress. The agreement is likely to be shattered when someone discovers that a set of

terms will not really work, perhaps in some hitherto neglected field, but this does not make agreement the key to workability. On the contrary, it is the other way round.

(3) The distinction between public and private is by no means the same as that between physical and mental. That is why methodological behaviorism (which adopts the first) is very different from radical behaviorism (which lops off the latter term in the second). The result is that while the radical behaviorist may in some cases consider private events (inferentially, perhaps, but none the less meaningfully), the Boring-Stevens operationist has maneuvered himself into a position where he cannot. 'Science does not consider private data,' says Boring. (Just where this leaves my contribution to the present symposium, I do not like to reflect.) But I contend that my toothache is just as physical as my typewriter, though not public, and I see no reason why an objective and operational science cannot consider the processes through which a vocabulary descriptive of a toothache is acquired and maintained. It is an amusing bit of irony that, while Boring must confine himself to an account of my external behavior, I am still reasonably interested in what might be called Boring-from-within.

(4) The public-private distinction apparently leads to a logical, as distinct from a psychological, analysis of the verbal behavior of the scientist, although I see no reason why it should. Perhaps it is because the subjectivist is still not interested in terms but in what the terms used to stand for. The only problem which a science of behavior must solve in connection with subjectivism is in the verbal field. How can we account for the behavior of talking about mental events? The solution must be psychological, rather than logical, and I have tried to suggest one approach in my present paper. The complete lack of interest in this problem among current psychological operationists is nicely demonstrated by the fact that the only other members of the present panel who seem to be interested in a *causal* analysis of verbal behavior are the two non-psychologists (one of them a logician!).

My reaction to this symposium, then, is two-fold. [The confusion which seems to have arisen from a principle which is supposed to eliminate confusion is discouraging.] But upon second thought it appears that the possibility of a genuine operationism in psychology has not yet been fully explored. With a little effort I can recapture my enthusiasm of fifteen years ago. (This is, of course, a private event.)

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